PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIX.

PART I.

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MDCCCXXXIX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

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upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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Sweden and Norway.

The Royal Academy of Sciences at Stockholm.
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The Lowndes's Professor of Astronomy, Cambridge.
The Plumian Professor of Astronomy, Cambridge.
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The Imperial Academy of Sciences at St. Petersburgh.
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The Observatory at Tubingen.
The Observatory at Wilna.
Professor Bessel, of Königsberg.
Dr. William Olbers, of Bremen.
The Dépôt de la Marine, Paris.
The Bowden College, United States.
The Waterville College, United States.

ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve of the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology and Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose also to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Geology or Mineralogy, communicated to the

Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose also to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose to give one of the Royal Medals in the year 1842 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1839, and prior to the termination of the Session in June 1842.

The Council propose also to give one of the Royal Medals in the year 1842 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1839, and prior to the termination of the Session in June 1842.

Adjudication of the Medals of the Royal Society for the year 1838 by His Royal Highness the President and Council.

The Rumford Medal to James David Forbes, Esq., F.R.S., for his "Experiments on the Polarization of Heat," published in the Transactions of the Royal Society of Edinburgh.

A Copley Medal to Professor K. F. Gauss, Forn. Memb. R.S., for his "Inventions and Mathematical Researches on Magnetism."

Another Copley Medal to Michael Faraday, Esq., D.C.L., F.R.S., for his "Researches on Specific Electrical Induction."

The Royal Medal, in the department of Chemistry, to Thomas Graham, Esq., M.A., F.R.S., for his paper entitled "Inquiries respecting the Constitution of Salts, of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides," published in the Philosophical Transactions for 1836.

The Royal Medal, in the department of Mathematics, to William Henry Fox Talbot, Esq., F.R.S., for his papers entitled "Researches in the Integral Calculus," published in the Philosophical Transactions for 1836 and 1837.

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PHILOSOPHICAL TRANSACTIONS.

I. *Experimental Researches in Electricity.—Fifteenth Series.*

By MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, Gottingen, Modena, Stockholm, &c. &c.*

Received November 15,—Read December 6, 1838.

§ 23. *Notice of the character and direction of the electric force of the Gymnotus.*

1749. WONDERFUL as are the laws and phenomena of electricity when made evident to us in inorganic or dead matter, their interest can bear scarcely any comparison with that which attaches to the same force when connected with the nervous system and with life; and though the obscurity which for the present surrounds the subject may for the time also veil its importance, every advance in our knowledge of this mighty power in relation to inert things, helps to dissipate that obscurity, and to set forth more prominently the surpassing interest of this very high branch of Physical Philosophy. We are indeed but upon the threshold of what we may, without presumption, believe man is permitted to know of this matter; and the many eminent philosophers who have assisted in making this subject known, have, as is very evident in their writings, felt up to the latest moment that such is the case.

1750. The existence of animals able to give the same concussion to the living system as the electrical machine, the voltaic battery, and the thunder storm, being with their habits made known to us by RICHER, S'GRAVESENDE, FIRMIN, WALSH, HUMBOLDT, &c. &c., it became of growing importance to identify the living power which they possess, with that which man can call into action from inert matter, and by him named electricity (265. 351.). With the *Torpedo* this has been done to perfection, and the direction of the current of force determined by the united and successive labours of WALSH*, CAVENDISH†, GALVANI‡, GARDINI§, HUMBOLDT and GAY-

* Philosophical Transactions, 1773, p. 461.

† Ibid. 1776, p. 196.

‡ ALDINI's Essai sur la Galvanism, ii. 61.

§ De Electrici ignis Natura, §. 71. Mantua, 1792.

LUSSAC*, TODD†, Sir HUMPHRY DAVY‡, Dr. DAVY§, BECQUEREL‖, and MATTEUCCI¶.

1751. The Gymnotus has also been experimented with for the same purpose, and the investigations of WILLIAMSON**, GARDEN††, HUMBOLDT‡‡, FAHLBERG§§ and GUISAN‖‖, have gone very far in showing the identity of the electric force in this animal with the electricity excited by ordinary means; and the two latter philosophers have even obtained the spark.

1752. As an animal fitted for the further investigation of this refined branch of science, the Gymnotus seems, in certain respects, better adapted than the Torpedo, especially (as HUMBOLDT has remarked) in its power of bearing confinement, and capability of being preserved alive and in health for a long period. A Gymnotus has been kept for several months in activity, whereas Dr. DAVY could not preserve Torpedos above twelve or fifteen days; and MATTEUCCI was not able out of 116 such fish to keep one living above three days, though every circumstance favourable to their preservation was attended to¶¶. To obtain Gymnoti has therefore been a matter of consequence; and being stimulated, as much as I was honoured, by very kind communications from Baron HUMBOLDT, I in the year 1835 applied to the Colonial Office, where I was promised every assistance in procuring some of these fishes, and continually expect to receive either news of them or the animals themselves.

1753. Since that time Sir EVERARD HOME has also moved a friend to send some Gymnoti over, which are to be consigned to His Royal Highness our late President; and other gentlemen are also engaged in the same work. This spirit induces me to insert in the present communication that part of the letter from Baron HUMBOLDT which I received as an answer to my inquiry of how they were best to be conveyed across the Atlantic. He says, "The Gymnotus, which is common in the Llanos de Caracas (near Calabozo), in all the small rivers which flow into the Orinoco, in English, French or Dutch Guiana, is not of difficult transportation. We lost them so soon at Paris because they were too much fatigued (by experiments) immediately after their arrival. MM. NORDERLING and FAHLBERG retained them alive at Paris above four months. I would advise that they be transported from Surinam (from Essequibo, Demerara, Cayenne) in summer, for the Gymnotus in its native country lives in water of 25° centigrade (or 77° FAHR.). Some are five feet in length, but I would advise that such as are about twenty-seven or twenty-eight inches in length be chosen. Their power varies with their food, and their state of rest. Having but a small stomach they eat little and often, their food being cooked meat, *not salted*,

* Annales de Chimie, xiv. 15.

‡ Ibid. 1829, p. 15.

‖ Traité de l'Électricité, iv. 264.

** Philosophical Transactions, 1775, p. 94.

‡‡ Personal Narrative, chap. xvii.

‖‖ De Gymnoto Electrico. Tubingen, 1819.

† Philosophical Transactions, 1816, p. 120.

§ Ibid. 1832, p. 259; and 1834, p. 531.

¶ Bibliothèque Universelle, 1837, tom. xii. 163.

†† Ibid. 1775, p. 102.

§§ Swedish Transactions, 1801, pp. 122. 156.

¶¶ Bibliothèque Universelle, 1837, xii. p. 174.

small fish, or even bread. Trial should be made of their strength and the fit kind of nourishment before they are shipped, and those fish only selected already accustomed to their prison. I retained them in a box or trough about four feet long, and sixteen inches wide and deep. The water must be *fresh*, and be changed every three or four days: the fish must not be prevented from coming to the surface, for they like to swallow air. A net should be put over and round the trough, for the Gymnotus often springs out of the water. These are all the directions that I can give you. It is, however, *important* that the animal should not be tormented or fatigued, for it becomes exhausted by frequent electric explosions. Several Gymnoti may be retained in the same trough."

1754. A Gymnotus has lately been brought to this country by Mr. PORTER, and purchased by the proprietors of the Gallery in Adelaide Street: they immediately most liberally offered me the liberty of experimenting with the fish for scientific purposes; they placed it for the time exclusively at my disposal, that (in accordance with HUMBOLDT's directions (1753.)) its powers might not be impaired: only desiring me to have a regard for its life and health. I was not slow to take advantage of their wish to forward the interests of science, and with many thanks accepted their offer. With this Gymnotus, having the kind assistance of Mr. BRADLEY of the Gallery, Mr. GASSIOT, and occasionally other gentlemen, as Professors DANIELL, OWEN and WHEATSTONE, I have obtained every proof of the identity of its power with common electricity (265. 351, &c.). All of these had been obtained before with the Torpedo (1750.), and some, as the shock, circuit, and spark (1751.), with the Gymnotus; but still I think a brief account of the results will be acceptable to the Royal Society, and I give them as necessary preliminary experiments to the investigations which we may hope to institute when the expected supply of animals arrives (1752.).

1755. The fish is forty inches long. It was caught about March 1838; was brought to the Gallery on the 15th of August, but did not feed from the time of its capture up to the 19th of October. From the 24th of August Mr. BRADLEY nightly put some blood into the water, which was changed for fresh water next morning, and in this way the animal perhaps obtained some nourishment. On the 19th of October it killed and eat four small fish; since then the blood has been discontinued, and the animal has been improving ever since, consuming upon an average one fish daily*.

1756. I first experimented with it on the 3rd of September, when it was apparently languid, but gave strong shocks when the hands were favourably disposed on the body (1760. 1773, &c.). The experiments were made on four different days, allowing periods of rest from a month to a week between each. His health seemed to improve continually, and it was during this period, between the third and fourth days of experiment, that he began to eat.

1757. Beside the hands two kinds of collectors were used. The one sort consisted each of a copper rod fifteen inches long, having a copper disc one inch and a half in diameter brazed to one extremity, and a copper cylinder to serve as a handle, with

* The fish eaten were gudgeons, carp, and perch.

large contact to the hand, fixed to the other, the rod from the disc upwards being well covered with a thick caoutchouc tube to insulate that part from the water. By these the states of particular parts of the fish whilst in the water could be ascertained.

1758. The other kind of collectors were intended to meet the difficulty presented by the complete immersion of the fish in water; for even when obtaining the spark itself I did not think myself justified in asking for the removal of the animal into air. A plate of copper eight inches long by two inches and a half wide, was bent into a saddle shape, that it might pass over the fish, and inclose a certain extent of the back and sides, and a thick copper wire was brazed to it, to convey the electric force to the experimental apparatus; a jacket of sheet caoutchouc was put over the saddle, the edges projecting at the bottom and the ends; the ends were made to converge so as to fit in some degree the body of the fish, and the bottom edges were made to spring against any horizontal surface on which the saddles were placed. The part of the wire liable to be in the water was covered with caoutchouc.

1759. These conductors being put over the fish, collected power sufficient to produce many electric effects; but when, as in obtaining the spark, every possible advantage was needful, then glass plates were placed at the bottom of the water, and the fish being over them, the conductors were put over it until the lower caoutchouc edges rested on the glass, so that the part of the animal within the caoutchouc was thus almost as well insulated as if the Gymnotus had been in the air.

1760. *Shock.* The shock of this animal was very powerful when the hands were placed in a favourable position, i. e. one on the body near the head, and the other near the tail; the nearer the hands were together within certain limits the less powerful was the shock. The disc conductors (1757.) conveyed the shock very well when the hands were wetted and applied in close contact with the cylindrical handles; but scarcely at all if the handles were held in the dry hands in an ordinary way.

1761. *Galvanometer.* Using the saddle conductors (1758.) applied to the anterior and posterior parts of the Gymnotus, a galvanometer was readily affected. It was not particularly delicate; for zinc and platina plates on the upper and lower surface of the tongue did not cause a permanent deflection of more than 25°; yet when the fish gave a powerful discharge the deflection was as much as 30°, and in one case even 40°. The deflection was constantly in a given direction, the electric current being always from the anterior parts of the animal through the galvanometer wire to the posterior parts. The former were therefore for the time externally positive, and the latter negative.

1762. *Making a magnet.* When a little helix containing twenty-two feet of silked wire wound on a quill was put into the circuit, and an annealed steel needle placed in the helix, the needle became a magnet, and the direction of its polarity in every case indicated a current from the anterior to the posterior parts of the Gymnotus through the conductors used.

1763. *Chemical decomposition.* Polar decomposition of a solution of iodide of potassium was easily obtained. Three or four folds of paper moistened in the solution

(322.) were placed between a platina plate and the end of a wire also of platina, these being respectively connected with the two saddle conductors (1758.). Whenever the wire was in conjunction with the conductor at the fore part of the Gymnotus, iodine appeared at its extremity; but when connected with the other conductor none was evolved at the place on the paper where it before appeared. So that here again the direction of the current proved to be the same as that given by the former tests.

1764. By this test I compared the middle part of the fish with other portions before and behind it, and found that the conductor A, which being applied to the middle was negative to the conductor B applied to the anterior parts, was, on the contrary, positive to it when B was applied to places near the tail. So that within certain limits the condition of the fish externally at the time of the shock appears to be such, that any given part is negative to other parts anterior to it, and positive to such as are behind it.

1765. *Evolution of heat.* Using a HARRIS's thermo-electrometer belonging to Mr. GASSIOT, we thought we were able in one case, namely, that when the deflection of the galvanometer was 40° (1761.), to observe a feeble elevation of temperature. I was not observing the instrument myself, and one of those who at first believed they saw the effect now doubts the result*.

1766. *Spark.* The electric spark was obtained thus. A good magneto-electric coil, with a core of soft iron wire, had one extremity made fast to the end of one of the saddle collectors (1758.), and the other fixed to a new steel file; another file was made fast to the end of the other collector. One person then rubbed the point of one of these files over the face of the other, whilst another person put the collectors over the fish, and endeavoured to excite it to action. By the friction of the files contact was made and broken very frequently; and the object was to catch the moment of the current through the wire and helix, and by breaking contact *during the current* to make the electricity sensible as a spark.

1767. The spark was obtained four times, and nearly all who were present saw it. That it was not due to the mere attrition of the two piles was shown by its not occurring when the files were rubbed together, independently of the animal. Since then I have substituted for the lower file a revolving steel plate, cut file fashion on its face, and for the upper file wires of iron, copper and silver, with all of which the spark was obtained†.

1768. Such were the general electric phenomena obtained from this Gymnotus whilst living and active in his native element. On several occasions many of them were obtained together; thus a magnet was made, the galvanometer deflected, and perhaps a wire heated, by one single discharge of the electric force of the animal.

* In more recent experiments of the same kind we could not obtain the effect.

† At a later meeting, at which attempts were made to cause the attraction of gold leaves, the spark was obtained directly between fixed surfaces, the inductive coil (1766.) being removed, and only short wires (by comparison) employed.

1769. I think a few further but brief details of experiments relating to the quantity and disposition of the electricity in and about this wonderful animal will not be out of place in this short account of its powers.

1770. When the shock is strong, it is like that of a large Leyden battery charged to a low degree, or that of a good voltaic battery of perhaps one hundred or more pair of plates, of which the circuit is completed for a moment only. I endeavoured to form some idea of the *quantity* of electricity by connecting a large Leyden battery (291.) with two brass balls, above three inches in diameter, placed seven inches apart in a tub of water, so that they might represent the parts of the Gymnotus to which the collectors had been applied; but to lower the intensity of the discharge, eight inches in length of six-fold thick wetted string were interposed elsewhere in the circuit, this being found necessary to prevent the easy occurrence of the spark at the ends of the collectors (1758.), when they were applied in the water near to the balls, as they had been before to the fish. Being thus arranged, when the battery was strongly charged and discharged, and the hands put into the water near the balls, a shock was felt, much resembling that from the fish; and though the experiments have no pretension to accuracy, yet as the tension could be in some degree imitated by reference to the more or less ready production of a spark, and after that the shock be used to indicate whether the quantity was about the same, I think we may conclude that a single medium discharge of the fish is at least equal to the electricity of a Leyden battery of fifteen jars, containing 3500 square inches of glass coated on both sides, charged to its highest degree (291.). This conclusion respecting the great quantity of electricity in a single Gymnotus shock, is in perfect accordance with the degree of deflection which it can produce in a galvanometer needle (367. 860. 1761.), and also with the amount of chemical decomposition produced (374. 860. 1763.) in the electrolyzing experiments.

1771. Great as is the force in a single discharge, the Gymnotus, as HUMBOLDT describes, and as I have frequently experienced, gives a double and even a triple shock; and this capability of immediately repeating the effect with scarcely a sensible interval of time, is very important in the considerations which must arise hereafter respecting the origin and excitement of the power in the animal. WALSH, HUMBOLDT, GAY LUSSAC, and MATTEUCCI have remarked the same thing of the Torpedo, but in a far more striking degree.

1772. As, at the moment when the fish wills the shock, the anterior parts are positive and the posterior parts negative, it may be concluded that there is a current from the former to the latter through every part of the water which surrounds the animal, to a considerable distance from its body. The shock which is felt, therefore, when the hands are in the most favourable position, is the effect of a very small portion only of the electricity which the animal discharges at the moment, by far the largest portion passing through the surrounding water. This enormous external current must be accompanied by some effect within the fish *equivalent* to a current, the direction

of which is from the tail towards the head, and equal to the sum of *all these external forces.* Whether the process of evolving or exciting the electricity within the fish includes the production of this internal current (which need not of necessity be as quick and momentary as the external one), we cannot at present say; but at the time of the shock the animal does not apparently feel the electric sensation which he causes in those around him.

1773. By the help of the accompanying diagram I will state a few experimental results which illustrate the current around the fish, and show the cause of the difference in character of the shock occasioned by the various ways in which the person is connected with the animal, or his position altered with respect to it. The large circle represents the tub in which the animal is confined; its diameter is forty-six inches, and the depth of water in it three inches and a half; it is supported on dry wooden legs. The figures represent the places where the hands or the disc conductors (1757.) were applied, and where they are close to the figure of the animal, it implies that contact with the fish was made. I will designate different persons by A, B, C, &c., A being the person who excited the fish to action.

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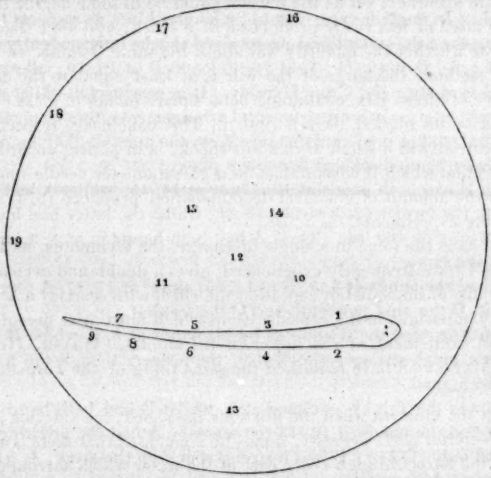


1774. When one hand was in the water the shock was felt in that hand only, whatever part of the fish it was applied to; it was not very strong, and was only in the part immersed in the water. When the hand and part of the arm was in, the shock was felt in all the parts immersed.

1775. When *both* hands were in the water at the *same* part of the fish, still the shock was comparatively weak, and only in the parts immersed. If the hands were on opposite sides, as at 1, 2, or at 3, 4, or 5, 6, or if one was above and the other below at the same part, the effect was the same. When the disc collectors were used in these positions no effect was felt by the person holding them, (and this corresponds with the observation of GAY-LUSSAC on Torpedos*,) whilst other persons, with both hands in at a distance from the fish, felt considerable shocks.

1776. When both hands or the disc collectors were applied at places separated by a part of the length of the animal, as at 1, 3, or 4, 6, or 3, 6, then strong shocks extending up the arms, and even to the breast of the experimenter, occurred, though another person with a single hand in at any of these places, felt comparatively little. The shock could be obtained at parts very near the tail, as at 8, 9. I think it was strongest at about 1 and 8. As the hands were brought nearer together the effect diminished, until being in the same cross plane, it was, as before described, only sensible in the parts immersed (1775.).

1777. B placed his hands at 10, 11, at least four inches from the fish, whilst A touched the animal with a glass rod to excite it to action; B quickly received a powerful shock. In another experiment of a similar kind, as respects the non-necessity of touching the fish, several persons received shocks independently of each other; thus A was at 4, 6; B at 10, 11; C at 16, 17; and D at 18, 19; all were shocked at once, A and B very strongly, C and D feebly. It is very useful, whilst experimenting with the galvanometer or other instrumental arrangements, for one person to keep his hands in the water at a moderate distance from the animal, that he may know and give information when a discharge has taken place.

1778. When B had both hands at 10, 11, or at 14, 15, whilst A had but one hand at 1, or 3, or 6, the former felt a strong shock, whilst the latter had but a weak one, though in contact with the fish. Or if A had both hands in at 1, 2, or 3, 4, or 5, 6, the effect was the same.

1779. If A had the hands at 3, 5, B at 14, 15, and C at 16, 17, A received the most powerful shock, B the next powerful, and C the feeblest.

1780. When A excited the Gymnotus by his hands at 8, 9, whilst B was at 10, 11, the latter had a much stronger shock than the former, though the former touched and excited the animal.

1781. A excited the fish by one hand at 3, whilst B had both hands at 10, 11 (or along), and C had the hands at 12, 13 (or across); A had the pricking shock in the immersed hand only (1774.); B had a strong shock up the arms; C felt but a slight effect in the immersed parts.

1782. The experiments I have just described are of such a nature as to require many repetitions before the general results drawn from them can be considered as established; nor do I pretend to say that they are anything more than indications of

* Annales de Chimie, xiv. p. 18.

the direction of the force. It is not at all impossible that the fish may have the power of throwing each of its four electric organs separately into action, and so to a certain degree direct the shock, i. e. he may have the capability of causing the electric current to emanate from one side, and at the same time bring the other side of his body into such a condition, that it shall be as a non-conductor in that direction. But I think the appearances and results are such as to forbid the supposition, that he has any control over the direction of the currents after they have entered the fluid and substances around him.

1783. The statements also have reference to the fish when in a straight form; if it assume a bent shape, then the lines of force around it vary in their intensity in a manner that may be anticipated theoretically. Thus if the hands were applied at 1, 7, a feebler shock in the arms would be expected if the animal were curved with that side inwards, than if it were straight, because the distance between the parts would be diminished, and the intervening water therefore conduct more of the force. But with respect to the parts *immersed*, or to animals, as fish *in the water* between 1 and 7, they would be more powerfully, instead of less powerfully, shocked.

1784. It is evident from all the experiments, as well as from simple considerations, that all the water and all the conducting matter around the fish through which a discharge circuit can in any way be completed, is filled at the moment with circulating electric power; and this state might be easily represented generally in a diagram by drawing the lines of inductive action (1231. 1304. 1338.) upon it: in the case of a Gymnotus, surrounded equally in all directions by water, these would resemble generally, in disposition, the magnetic curves of a magnet, having the same straight or curved shape as the animal, i. e. provided he, in such cases, employed, as may be expected, his four electric organs at once.

1785. This Gymnotus can stun and kill fish which are in very various positions to its own body; but on one day when I saw it eat, its action seemed to me to be peculiar. A live fish about five inches in length, caught not half a minute before, was dropped into the tub. The Gymnotus instantly turned round in such a manner as to form a coil inclosing the fish, the latter representing a diameter across it; a shock passed, and there in an instant was the fish struck motionless, as if by lightning, in the midst of the waters, its side floating to the light. The Gymnotus made a turn or two to look for its prey, which having found he bolted, and then went searching about for more. A second smaller fish was given him, which being hurt in the conveyance, showed but little signs of life, and this he swallowed at once, apparently without shocking it. The coiling of the Gymnotus round its prey had, in this case, every appearance of being intentional on its part, to increase the force of the shock, and the action is evidently exceedingly well suited for that purpose (1783.), being in full accordance with the well-known laws of the discharge of currents in masses of

conducting matter; and though the fish may not always put this artifice in practice, it is very probable he is aware of its advantage, and may resort to it in cases of need.

1786. Living as this animal does in the midst of such a good conductor as water, the first thoughts are thoughts of surprise that it can sensibly electrify anything, but a little consideration soon makes one conscious of many points of great beauty, illustrating the wisdom of the whole arrangement. Thus the very conducting power which the water has; that which it gives to the moistened skin of the fish or animal to be struck; the extent of surface by which the fish and the water conducting the charge to it are in contact; all conduce to favour and increase the shock upon the doomed animal, and are in the most perfect contrast with the inefficient state of things which would exist if the Gymnotus and the fish were surrounded by air; and at the same time that the power is one of low intensity, so that a dry skin wards it off, though a moist one conducts it (1760.): so is it one of great quantity (1770.), that though the surrounding water does conduct away much, enough to produce a full effect may take its course through the body of the fish that is to be caught for food, or the enemy that is to be conquered.

1787. Another remarkable result of the relation of the Gymnotus and its prey to the medium around them is, that the larger the fish to be killed or stunned, the greater will be the shock to which it is subject, though the Gymnotus may exert only an equal power; for the large fish has passing through its body those currents of electricity, which, in the case of a smaller one, would have been conveyed harmless by the water at its sides.

1788. The Gymnotus appears to be sensible when he has shocked an animal, being made conscious of it, probably, by the *mechanical impulse* he receives, caused by the spasms into which it is thrown. When I touched him with my hands, he gave me shock after shock; but when I touched him with glass rods, or the insulated conductors, he gave one or two shocks, felt by others having their hands in at a distance, but then ceased to exert the influence, as if made aware it had not the desired effect. Again, when he has been touched with the conductors several times, for experiments on the galvanometer or other apparatus, and appears to be languid or indifferent, and not willing to give shocks, yet being touched by the hands, they, by convulsive motion, have informed him that a sensitive thing was present, and he has quickly shown his power and his willingness to astonish the experimenter.

1789. It has been remarked by Geoffroy St. Hilaire, that the electric organs of the Torpedo, Gymnotus, and similar fishes, cannot be considered as essentially connected with those which are of high and direct importance to the life of the animal, but to belong rather to the common teguments; and it has also been found that such Torpedos as have been deprived of the use of their peculiar organs, have continued the functions of life quite as well as those in which they were allowed to remain.

These, with other considerations, lead me to look at these parts with a hope that they may upon close investigation prove to be a species of natural apparatus, by means of which we may apply the principles of *action and re-action* in the investigation of the nature of the *nervous influence*.

1790. The anatomical relation of the nervous system to the electric organ; the evident exhaustion of the nervous energy during the production of electricity in that organ; the apparently equivalent production of electricity in proportion to the quantity of nervous force consumed; the constant direction of the current produced, with its relation to what we may believe to be an equally constant direction of the nervous energy thrown into action at the same time; all induce me to believe, that it is not impossible but that, on passing electricity per force through the organ, a reaction back upon the nervous system belonging to it might take place, and that a restoration, to a greater or smaller degree, of that which the animal expends in the act of exciting a current, might perhaps be effected. We have the analogy in relation to heat and magnetism. SEEBECK taught us how to commute heat into electricity; and PELTIER has more lately given us the strict converse of this, and shown us how to convert the electricity into heat, including both its relation of hot and cold. OERSTED showed how we were to convert electric into magnetic forces, and I had the delight of adding the other member of the full relation, by reacting back again and converting magnetic into electric forces. So perhaps in these organs, where nature has provided the apparatus by means of which the animal can exert and convert nervous into electric force, we may be able, possessing in that point of view a power far beyond that of the fish itself, to re-convert the electric into the nervous force.

1791. This may seem to some a very wild notion, as assuming that the nervous power is in some degree analogous to such powers as heat, electricity, and magnetism. I am only assuming it, however, as a reason for making certain experiments, which, according as they give positive or negative results, will regulate further expectation. And with respect to the nature of nervous power, that exertion of it which is conveyed along the nerves to the various organs which they excite into action, is not the direct principle of *life*; and therefore I see no natural reason why we should not be allowed in certain cases to *determine* as well as observe its course. Many philosophers think the power is electricity. PRIESTLEY put forth this view in 1774 in a very striking and distinct form, both as regards ordinary animals and those which are electric, like the Torpedo*. Dr. WILSON PHILIP considers that the agent in certain nerves is electricity modified by vital action†. MATTEUCCI thinks that the nervous

* PRIESTLEY on Air, vol. i. p. 277. Edition of 1774.

† Dr. WILSON PHILIP is of opinion, that the nerves which excite the muscles and effect the chemical changes of the vital functions, operate by the electric power supplied by the brain and spinal marrow, in its effects, modified by the vital powers of the living animal; because he found, as he informs me, as early as 1815, that while the vital powers remain, all these functions can be as well performed by voltaic electricity after the removal of the nervous influence, as by that influence itself; and in the end of that year he presented a paper to the Royal Society, which was read at one of their meetings, giving an account of the experiments on which this position was founded.

fluid or energy, in the nerves belonging to the electric organ at least, is electricity*. MM. PREVOST and DUMAS are of opinion that electricity moves in the nerves belonging to the muscles; and M. PREVOST adduces a beautiful experiment, in which steel was magnetized, in proof of this view; which, if it should be confirmed by further observation and by other philosophers, is of the utmost consequence to the progress of this high branch of knowledge†. Now though I am not as yet convinced by the facts that the nervous fluid is only electricity, still I think that the agent in the nervous system may be an inorganic force; and if there be reasons for supposing that magnetism is a higher relation of force than electricity (1664. 1731. 1734.), so it may well be imagined, that the nervous power may be of a still more exalted character, and yet within the reach of experiment.

1792. The kind of experiment I am bold enough to suggest is as follows. If a Gymnotus or Torpedo has been fatigued by frequent exertion of the electric organs, would the sending of currents of similar force to those he emits, or of other degrees of force, either continuously or intermittingly in the same direction as those he sends forth, restore him his powers and strength more rapidly than if he were left to his natural repose?

1793. Would sending currents through in the contrary direction exhaust the animal rapidly? There is, I think, reason to believe that the Torpedo (and perhaps the Gymnotus) is not much disturbed or excited by electric currents sent only through the electric organ; so that these experiments do not appear very difficult to make.

1794. The disposition of the organs in the Torpedo suggest still further experiments on the same principle. Thus when a current is sent in the natural direction, i. e. from below upwards through the organ on one side of the fish, will it excite the organ on the other side into action? or if sent through in the contrary direction, will it produce the same or any effect on that organ? Will it do so if the nerves proceeding to the organ or organs be tied? and will it do so after the animal has been so far exhausted by previous shocks as to be unable to throw the organ into action in any, or in a similar, degree of his own will?

1795. Such are some of the experiments which the conformation and relation of the electric organs of these fishes suggest, as being rational in their performance, and promising in anticipation. Others may not think of them as I do; but I can only say for myself, that were the means in my power, they are the very first that I would make.

* Bibliothèque Universelle, 1837, tom. xii. 192.

† Ibid, 1837, xii. 202; xiv. 200.

Royal Institution,
November 9th, 1838.

II. *On the Application of the Conversion of Chlorates and Nitrates into Chlorides, and of Chlorides into Nitrates, to the determination of several equivalent numbers.* *By* Frederick Penny, *Esq.* *Communicated by* H. Hennell, *Esq. F.R.S.*

Received January 17,—Read January 24, 1839.

1. THE following researches originated from some experiments which were undertaken to discover an improved method for ascertaining the quantity of nitrate of potassa existing in crude saltpetre. After several unsuccessful attempts the action of hydrochloric acid was tried. The fact, that nitrates are decomposed by this acid, has been long known; but the nature of the resulting compound of potassium has not, so far as I am aware, been hitherto determined. I anticipated that the nitrate would be decomposed into chloride of potassium. To decide the question some pure nitrate of potassa was mixed with hydrochloric acid, and the mixture heated; while at common temperatures no perceptible action occurs, but immediately the acid becomes hot, decomposition commences. Chlorine and nitrous acid are evolved with copious effervescence, and the nitrate slowly disappears. The solution was gradually evaporated to dryness, and the dry salt treated with an additional quantity of acid until decomposition was no longer evident. The resulting salt was then carefully examined, and it was found to be pure chloride of potassium. This experiment was repeated several times, and all the results concurred in satisfactorily establishing the fact, that nitrate of potassa may be perfectly converted into chloride of potassium, provided a sufficient quantity of the acid be employed, and the temperature necessary to effect the decomposition be properly regulated.

2. So far the decomposition was admirably adapted for the object mentioned at the commencement. The usual impurities, such as chlorides, sulphates, silica, &c. which any sample of crude saltpetre might contain, would obviously remain unchanged, while the nitrate of potassa alone suffering decomposition, its quantity could easily be ascertained, by comparing the weight of the resulting salt with that obtained from a known quantity of absolutely pure nitrate. Several experiments were therefore performed to determine the exact quantity of chloride of potassium corresponding to a known weight of nitrate. The mean result of four experiments gave the ratio of 100 of nitrate to 73·730 chloride. I was then naturally led to compare this result with the equivalent numbers of these two compounds. In this country there are two series of equivalents in general use, one in which whole numbers are adopted, and the other in which fractional parts are admitted. For example, according to the former, nitrate of potassa will be 102, and to the latter 101·3. So chlo-

ride of potassium will be 76 and 74·6. Whence, according to the former, every 100 parts of nitrate should yield 74·51 of chloride: the latter gives the ratio of 100 to 73·613. But these results differ considerably from my experiments. In the one case, we have a difference of ·78, and in the other ·12. Whence therefore could they arise? Either the process must be defective, or the equivalent numbers, so generally considered as correct, must be erroneous.

3. To determine whether the process was inaccurate, I tried if chloride of potassium could be converted into nitrate of potassa by nitric acid. A single experiment upon some pure chloride decided in the affirmative; and the mean result of three experiments gave the ratio of 100 of nitrate to 73·727 of chloride. The correctness, therefore, of the experiments upon the nitrate was satisfactorily confirmed. Moreover in another experiment I converted a known weight of nitrate into chloride by hydrochloric acid, and then reconverted the resulting chloride into nitrate by nitric acid. The quantity of nitrate obtained was very nearly the same as that originally employed; there was not a hundredth of a grain difference. The error was thus traced to the equivalent numbers, but whether it existed in one, or more, of the elementary bodies constituting the above compounds, remained still unknown. The subject cannot be decided by the mere conversion of a nitrate into a chloride, or a chloride into a nitrate. Additional data are required. In order therefore to examine the question more scrupulously, the following experiments were undertaken; and as the results have proved consistent and satisfactory, I have presumed to submit them to the Royal Society.

4. In the present communication I propose to examine the equivalent numbers of oxygen, chlorine, nitrogen, potassium, sodium, and silver; and the following are the successive steps by which this examination has been conducted.

5. In the first place the equivalent of chloride of potassium will be determined, by decomposing the chlorate of potassa into oxygen, and chloride of potassium. From the ratio which these results bear to each other, the number for chloride of potassium, as compared with oxygen, may be easily deduced.

The same results will also give the number for chlorate of potassa.

Secondly. The equivalent of nitrate of potassa is ascertained. This is effected by converting the chlorate of potassa and the chloride of potassium into nitrate: and as we have already established the equivalents of chlorate and chloride, we can easily calculate the number for nitrate from these two methods of analysis. Moreover, from the equivalents of chlorate and nitrate of potassa, we can also learn the difference between the equivalents of chlorine and nitrogen, as the two salts agree exactly in composition, and only differ by nitrogen in the one being substituted for chlorine in the other. To confirm the experiments upon the conversion of chloride of potassium into nitrate, I have detailed some upon the decomposition of nitrate into chloride.

The process, hereafter described, by which these experiments were performed, was

so exceedingly simple, that the preceding differences could not be referable to errors of manipulation.

Thirdly. The equivalents of nitrate, chlorate, and chloride of sodium are resolved. The method of investigation is similar to that adopted for the analogous salts of potassium. The chlorate of soda has been decomposed into chloride, and into nitrate; the nitrate of soda into chloride; and the chloride of sodium into nitrate of soda. The results of all these experiments are perfectly consistent with each other, and they confirm in the most satisfactory manner the accuracy of the experiments upon the salts of potassium.

Fourthly. The equivalent numbers of chlorine, nitrogen, potassium, and sodium have been ascertained. For this purpose the metal silver has been selected. The salts of potassium or sodium do not admit of application in this respect, as it is impracticable to ascertain with sufficient accuracy for the present purpose, the proportions in which either of these metals combine with chlorine or nitric acid. Bismuth was first selected from the facility of converting it into oxide, but the difficulty of obtaining this metal in a state of absolute purity, and its liability to form subsalts, induced me to prefer silver. I have accordingly determined the exact proportions in which this metal combines with chlorine and nitric acid; and lastly, to prove the correctness of the relation which the resulting nitrate bears to the chloride, I have performed several experiments upon the conversion of the crystallized nitrate into chloride. Perfect consistency exists throughout; and from these results, conjoined with those from the salts of potassium and sodium, the equivalents of chlorine, nitrogen, potassium, sodium, and silver are easily deduced.

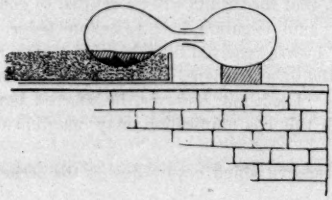
6. Such then is the arrangement of the evidence by which I propose to determine the equivalent numbers of these six elementary bodies; and to show that a slight alteration is necessary in those which are generally employed. Considering the number of distinguished chemists who have devoted their time and ability to the subject, it might naturally be expected that the truth had been satisfactorily established; but the slightest observation of the discrepancies in the results obtained, will prove the urgency of more refined investigations. In offering my feeble evidence, I am truly sensible of the responsible task I have undertaken. The well-attested skill of those from whom I shall have occasion to differ has constantly urged me to seek every means calculated to attain truth; and I trust, from the confirmatory nature of the evidence advanced, the numerous experiments performed, and the simplicity of the processes employed, that the results will be entitled to confidence.

7. In presenting this communication, I beg to acknowledge, with sincere gratitude, the constant assistance I have received from Mr. Hennell; he has kindly watched the whole progress of the investigations, and by his advice has enabled me to overcome several difficulties. Let it not, however, be inferred that he is responsible for any of the faults which this communication may contain: they must rest entirely on myself.

8. The violent effervescence which occurs during the action of nitric acid upon a

chloride, or of hydrochloric acid upon a nitrate, except in the case of silver, appeared, at first, to offer an insuperable difficulty to the application of these actions to so delicate a purpose as the determination of equivalent numbers. It was requisite that some simple practical method should be devised to obviate this source of error. The unavoidable loss occurring during effervescence has rendered many chemical processes, which promise the most favourable results in theory, inapplicable to important investigations. In the experiments hereafter to be described, any error from this cause has been effectually prevented. A loss by spirting cannot occur. The substance is operated upon in a common flask placed in a horizontal position, with another attached to it as a receiver; the resulting substance is dried; and all the weighings are accomplished without removal from the flask. The performance and completion of the operation in one vessel is an additional advantage, and one which renders the process especially applicable to minute chemical researches. All those operations, such as transference, washing, filtering, &c. which introduce so many errors, are avoided. Little comment is required to show the advantages which the above process possesses in comparison to those hitherto applied to the determination of equivalent numbers. Few improvements, I imagine, can more increase the precision of our analyses, than a reduction of the number of operations by which these analyses are accomplished, combined with the employment of vessels which will bear a high temperature, resist the action of acids, and admit of being used with the most delicate balances. All these conditions have been fulfilled by the process employed in these researches, and as it has been applied to all the experiments, I shall premise its details thus early, to avoid unnecessary repetition.

9. Into a Florence flask, previously washed, dried, and tared, with the precautions hereafter stated, the substance to be operated upon was introduced, and its weight carefully ascertained. Another flask, with the whole of its neck removed, was also tared; and, as this was intended to be adapted to the other as a receiver, it was termed the receiver flask. The requisite quantity of acid was then poured upon the weighed substance, and the flask immediately placed in a horizontal position. The receiver flask was also adapted in the same position. If the decomposition would commence and continue without the assistance of heat, the flasks were allowed to remain as thus placed; but if heat were necessary, they were transferred without breaking the connexion to a sand bath. The arrangement is represented in the accompanying figure, and from an inspection of this, it can readily be perceived that a



loss by spirting cannot occur. The drops of liquid, being ejected vertically by the effervescence, are thrown against the upper side of the flask. Should any drops be thrown laterally, a circumstance of rare occurrence, they will be secured by the receiver. When the decomposition is completed, and the action has entirely ceased, the receiver is detached, and the solution cautiously evaporated to dryness. The receiver in the meantime is dried and re-weighed. During the evaporation of the excess of acid, a portion of the vapour condenses upon the upper side of the flask, and, returning to the solution, carries with it any salt that may have been thrown to the top. Moreover, from this internal washing of the flask, as it may not inaptly be termed, continuing during the whole of the evaporation, the dry salt, instead of being scattered over a considerable portion of the flask, is reduced to a small compact mass. Indeed it is beautiful to observe the small compass which the dry nitrates occupy from this cause. The flask and its contents are lastly heated in the flame of a spirit lamp, and the dry salt either fused or not according to its nature. After a sufficient time has been allowed for the cooling of the flask, it is returned to the balance, and the weight of the resulting compound carefully ascertained.

10. From the preceding process it will be remarked, that flasks are the only vessels which have been employed. Many precautions must be observed to ensure their successful employment. Those that I have used are of the capacity of a pint, and the lightest were always selected. Their average weight was about 320 grains. Perfect cleanliness was obtained by boiling nitric acid, and well rinsing with common, and afterwards with distilled water. They were dried by heating on a sand bath, and then in the flame of a spirit lamp. I have found it advantageous to remove one half the necks; for by this, their weights are materially lessened, and their dryness is more securely ensured. While cooling they were placed inside a glass case, free from dust, and were allowed to remain for two hours, in order to restore the equilibrium of the internal and external air. In ascertaining the weights of the flasks, it is better to weigh them against another flask, previously selected and dried for the purpose. This latter flask may be termed the gauge flask. It weighed 323·420 grains, and weights were of course added, on either side, to adjust the difference between it and the flask to be tared. The gauge flask was frequently reheated to insure accuracy, and when not in use was preserved from dust and damp under a bell glass placed inside a glass case. Before the final observations of the weighings were noted, the flasks were allowed to remain in the balance for a quarter of an hour. The success of the operations depends especially upon a strict observance of these precautions. The following circumstance will serve as an ample proof of the care required. Having observed in one of the experiments a small particle of dust upon the flask, I removed it from the balance, and wiped it as quickly as possible once round with a silk pocket handkerchief. The flask was held during this operation by the very extremity of the neck. Upon returning it to the scale it had lost ·025 of a grain, and this was owing to a slight expansion of the internal air, produced by the heat of the hand. It re-

gained its original weight in less than five minutes. By repeating this operation several times the flask lost nearly two tenths. The scales employed were made by Mr. ROBINSON, and would turn decidedly with the thousandth part of a grain, when loaded with five hundred grains. In the following experiments they were never loaded with so much. The weights were new, and by careful examination beforehand, were ascertained to be consistent.

Chlorate of Potassa.

11. It has already been stated, that the object with this salt, is its decomposition into oxygen and chloride of potassium. The method of accomplishing this hitherto has been to submit the salt to a high temperature; by which the oxygen is expelled and chloride of potassium remains. There are, however, many difficulties which render this process objectionable, and therefore I was induced to inquire whether the same result could be obtained by the action of hydrochloric acid. The changes occurring during the decomposition are well known. When the chlorate is in excess, the euchlorine of Sir HUMPHRY DAVY, or, as since proved by SOUBEIRAN, a mixture of chlorine and chlorous acid, is evolved. If, however, the acid be in excess, chlorine only will be given off. All the oxygen of the chloric acid, and of the potassa, unites with the hydrogen of the hydrochloric acid, and forms water. The potassium remains combined with chlorine. I satisfied myself by numerous experiments that chloride of potassium is alone produced; and being enabled, by the process already described (9.), to counteract any error from the effervescence, I have adopted this method by hydrochloric acid in preference to that by heat.

12. The chlorate of potassa employed in the succeeding experiments was obtained pure by three successive crystallizations of ordinary commercial chlorate. After each crystallization the crystals were dried, rubbed to powder, and washed with distilled water. The salt which resulted from the third crystallization, dissolved without residue in water, and the solution was not affected by nitrate of silver or nitrate of baryta. Two hundred grains were converted by heat into chloride of potassium; the fused chloride was perfectly colourless, and when dissolved and tested not a trace of foreign matter could be detected.

13. The first experiments with this salt were rather discrepant, arising from my ignorance of the extreme caution which is requisite in drying it. Having observed in some chemical works that it is fusible at a temperature between 400° and 500° FAHR., and will bear a temperature of 660° without undergoing decomposition, I conceived that very careful fusion would be the most secure method for ensuring dryness. The want of uniformity in the results, however, indicated some source of error, and upon examination I found that a very notable quantity of chloride of potassium was produced by the process of fusion, although effected with the utmost care. A temperature just below that of fusion was next tried, but upon dissolving the dry salt in distilled water, and testing with nitrate of silver, a minute portion of chloride could

be detected. Indeed it was not until after several trials that I succeeded in ascertaining the temperature at which this salt might be perfectly dried without risk of decomposition. This temperature is about 220°. The temperature of boiling water is quite safe: 1000 grains were kept in a copper pan, heated by steam, for fourteen hours without experiencing the slightest change. I have found by several experiments that one hundred grains of perfectly dried chlorate will lose about ·05 of a grain by fusion. The precaution of testing a portion of the dry salt was repeated after every drying, to ensure accuracy.

14. When dry, the powder was transferred, while hot, into a small tube, and tightly corked. As soon as cold, the quantity necessary for an experiment was transferred into a previously tared flask, and the weight of it ascertained. The tube was sufficiently small to pass down the neck of the flask, and almost to the bottom, so that any adhesion of the powder to the sides of the flask was prevented. The remainder of the process corresponded to the one already described, except that before adding the hydrochloric acid, it is expedient to fuse the chlorate in the flask. By this the action is very much moderated, and the chlorate is slowly and quietly dissolved. No heat is requisite if the acid be sufficiently strong.

15. The hydrochloric acid was prepared according to the general directions in a green glass retort. It was twice redistilled, and had a specific gravity of 1·160. It was carefully examined for sulphuric, nitric, and sulphurous acids, but not a trace of them could be detected. Two fluid ounces when evaporated left only ·015 of a grain of solid matter*.

16. Owing to the compactness of the mass in which the resulting chloride is left in the flask, a minute portion of hydrochloric acid remains adherent to the salt when its quantity is large; and this cannot be driven off without so high a temperature as to risk the cracking of the flask. To expel this adventitious acid, I have always adopted the following process. The mass, the weight of which has been ascertained, is detached from the side of the flask by gentle agitation, and broken into pieces sufficiently small to be removed. These pieces are then rubbed to powder in a glass mortar, and the powder is transferred into a previously tared platina crucible. Its weight is ascertained, and then it is heated to dull redness for half an hour. Fusion was carefully avoided, as it cannot be effected without a minute loss by sublimation. When cold the loss is easily determined, and as the weight of the salt in the crucible is known, it is easy to calculate the loss for the whole quantity originally in the flask. The quantity which remained adherent to the flask generally amounted to ·07 or ·08 of a grain.

17. I subjoin the results of six experiments, and annex, for the convenience of comparison, the calculations to 100 parts.

* In the following experiments one fluid ounce of this acid was sufficient, so that not more than ·008 of a grain of impurity would be introduced, and this quantity was always subtracted from the resulting chloride.

Chlorate.	Chloride.	Chlorate.	Chloride.
76·626 . . .	46·598 . . .	or as 100 to . . .	60·825
82·048 . . .	49·903 . . .	or as 100 to . . .	60·822
75·200 . . .	45·733 . . .	or as 100 to . . .	60·815
63·114 . . .	38·386 . . .	or as 100 to . . .	60·820
61·164 . . .	37·202 . . .	or as 100 to . . .	60·823
65·724 . . .	39·980 . . .	or as 100 to . . .	60·830

The mean of these experiments indicates that 100 parts of chlorate of potassa contain 60·823 of chloride of potassium, and 39·177 of oxygen.

18. In another experiment I obtained the proportion of 100 of chlorate to 60·808 of chloride; but in this case the chloride was partially fused, and while so I detected the evolution of a small quantity of white vapour. It shows that 60·808 is too low. Berzelius obtained by heat 60·850 of chloride, and 39·150 of oxygen from 100 of chlorate.

19. It being well known that many powders, although not deliquescent, are more or less hygrometric, and no precautions having been taken to obviate this in the preceding experiments, I thought an error on this account might influence them. To decide the query, the following experiment was performed. Two hundred grains of the chlorate in powder were dried with every care, and transferred while hot into a stoppered phial, just large enough to contain it. When quite cold, the stopper was removed for a moment, and then the bottle and its contents, being placed in a small glass dish, the whole was counterpoised in the balance. The powder was transferred very carefully into the dish, without removal from the balance, and the empty bottle placed on top of the powder. The whole was left in this condition for two hours, and upon examination at the expiration of that time, not the slightest appreciable difference in weight could be detected. The experiment was twice repeated with the same result.

20. Perfectly dry powdered chloride of potassium was submitted to a similar experiment, but there was no evidence of change. If however the atmosphere be damp, both these salts when in powder rapidly absorb hygrometric moisture.

Conversion of Chlorate of Potassa into Nitrate.

21. Having determined, and I trust satisfactorily, that every 100 parts of chlorate correspond to 60·823 of chloride, the next object was to ascertain the quantity of nitrate resulting from 100 parts of the same salt. Its conversion into nitrate cannot however be performed directly, for the action of nitric acid upon a chlorate, as I shall show at another opportunity, is not to decompose it wholly into nitrate, but into a definite mixture of nitrate and perchlorate. The means by which the object now desired was accomplished, were by converting the chlorate into chloride, by hydrochloric acid, and then the resulting chloride into nitrate, by nitric acid. The operations were performed without any removal from the flask. The nitric acid was

added immediately the chloride was dry, without transferring and driving off the small quantity of adhering acid (16.). The minuteness of its quantity rendered it of no importance, as considerable excess of nitric acid was always employed. The nitrate produced was always fused to ensure dryness, and its weight being ascertained, it was dissolved and tested for chlorate and chloride. There was never the slightest indication of either. The impurities introduced by the acids, namely, one ounce of hydrochloric, and half an ounce of nitric, were of course allowed for. Only four experiments were performed, as their uniformity assured me of their close approximation to the truth.

22. The particulars of them are as follows:

Chlorate.	Nitrate.	Chlorate.	Nitrate.
86·454	71·329	100	82·505
70·205	57·918	100	82·497
62·244	51·368	100	82·498
72·636	59·938	100	82·500

The mean will be as 100 chlorate to 82·500 of nitrate.

23. Hereafter I shall show that 100 parts of nitrate correspond to 73·726 of chloride, so that the above result would indicate that 100 of chlorate contain 60·825 of chloride. This agrees very closely with the experiments by hydrochloric acid.

24. Moreover, this result, by comparison with that obtained in 17, shows, that

100 parts of chloride correspond to 135·640 of nitrate
and 100 parts of nitrate correspond to 73·724 chloride.

Chloride of Potassium.

25. Notwithstanding the experiments which have been made upon the action of nitric acid upon chlorides, much uncertainty still exists, as to the precise nature of the changes which occur. Their explication would dispel the doubt respecting the changes which take place during the mutual decomposition of nitric and hydrochloric acid, in the formation of nitro-muriatic acid. Sir H. Davy has stated that chlorine, nitrous acid, and water are produced. According to Dr. John Davy, a compound of chlorine and binoxide of nitrogen, with chlorine, and water result. Interesting as the decision of this question would be, I shall not now adduce the experiments I have made for this purpose, lest I should confuse the present inquiry. It has reference only to the ultimate result, which, as before stated (3.), is proved by the following experiment to be pure nitrate of potassa.

26. Two hundred grains of pure fused chloride of potassium were acted upon by one fluid ounce of nitric acid, sp. gr. 1·425; effervescence began immediately on mixture: abundance of chlorine tinged with nitrous acid was evolved. After the action had continued for two hours, a gentle heat was applied. There was a more copious evolution of nitrous acid, but the action soon ceased, and the solution remained co-

lourless. Evaporation was carried to dryness, and the dry salt fused. It was colourless, and weighed about 271·24. This corresponds closely to the quantity of nitrate which should be yielded according to the preceding experiments. Part of the fused salt was then dissolved in distilled water, and tested with nitrate of silver: not the slightest turbidity was evident. To another portion hydrochloric acid was added, to observe whether any chlorate of potassa had been formed, but not the slightest change was produced. The remaining portion upon being crystallized proved to be all nitre.

27. The chloride employed was prepared by saturating pure bicarbonate of potassa with excess of pure hydrochloric acid. The bicarbonate was purified by three crystallizations, and when tested no indication of foreign matter could be detected. The solution of chloride was evaporated to dryness, and the dry salt heated to expel the excess of acid. It was then dissolved in distilled water, and twice recrystallized. The salt, thus purified, was examined for sulphate, nitrate, lime, &c., but no impurity could be discovered. It was dried by careful fusion in the platina crucible. The process has already been detailed. Only half a fluid ounce of the nitric acid was employed in the following experiments. This nitric acid had been twice distilled and was perfectly colourless. It had a specific gravity 1·430. Two fluid ounces when evaporated left ·020 of a grain, and therefore not more than ·005 of a grain of impurity would be introduced.

28. The following are the particulars of the experiments performed:

Chloride.	Nitrate.	Chloride.	Nitrate.
84·090	113·058	100	135·639
61·998	84·092	100	135·637
51·823	70·293	100	135·640
67·145	91·072	100	135·635
55·350	75·071	100	135·630

whence, according to the mean,

100 of chloride correspond to 135·634 nitrate,
and 100 of nitrate correspond to 73·726 chloride.

29. In confirmation of these results, I procured some chloride from pure chlorate, by heating in the platina crucible, and fusing the resulting salt. I performed two experiments with this chloride and obtained the annexed results:

Chloride.	Nitrate.		
38·660	52·438	100	135·640
60·998	82·732	100	135·630

They correspond very closely to the preceding.

Nitrate of Potassa.

30. The conversion of this salt into chloride of potassium has been described, as well as the process by which it is effected. In addition to the precautions mentioned

in the process, there are several others to be observed to ensure success. As soon as the action commences, the flask should be removed from the sand bath, and the decomposition be permitted to continue without heat. When the action slackens, the flask may be returned to the sand bath, and so on, to and from the source of heat, until it is so gentle that no lateral spirting can be feared. The adaptation of a receiver throughout the whole of these operations must be strictly attended to; for despite every care, a minute portion of the solution will sometimes be ejected during the evaporation. This circumstance happened in three of the experiments I shall mention presently. In one case the weight was ·003 of a grain, in another ·008, and in a third ·005. The receiver flask should not be detached from the other even while removing them to and from the sand bath, nor in fact until the resulting chloride is nearly dry, as slight action continues during the greater part of the evaporation.

31. Very particular attention was paid to the purity of the salt employed. It was prepared from the common purified nitre of the shops, by four repeated crystallizations from distilled water. After each crystallization the salt was dried, and finely powdered, and this powder well washed with distilled water. When finally washed and dried, 200 grs. were fused in a platina crucible. They yielded a perfectly colourless mass, soluble in water without a trace of insoluble matter, and the solution was not affected by nitrate of silver, chloride of barium, oxalate of ammonia, or hydrosulphuret of ammonia. It was dried by careful fusion. The resulting chloride was dried in the same way as the chloride from chlorate. From one to two fluid ounces of hydrochloric acid are sufficient for the quantities of nitrate employed. In adding this acid one important practical circumstance must be observed, namely to add it in two successive portions, and to expel the first quantity previously to the addition of the second. This method is necessary from the small size of the flask, compared with the bulk of fluid. The less the quantity of fluid in the flask at one time, the less chance of loss by spirting.

32. I obtained, in seven experiments, the following very satisfactory results:

Nitrate.	Chloride.	Nitrate.	Chloride.
60·290	44·452	100	73·731
58·230	42·927	100	73·726
63·040	46·479	100	73·730
51·514	37·978	100	73·724
56·036	41·315	100	73·730
80·517	59·362	100	73·726
78·643	57·983	100	73·730

The mean will be as

100 of nitrate to 73·728 chloride,
or 100 of chloride to 135·634 nitrate.

33. Thus then we have ascertained, by three different methods, the ratio which chloride of potassium bears to nitrate of potassa. The results are as follows: from chlorate potassa (17.),

Nitrate.		Chloride.		Chloride.
100 corresponds to		73·724;	or 100 to	135·640.

From chloride potassium (28.),

100 corresponds to 73·726; or 100 to 135·634.

From nitrate of potassa (32.),

100 corresponds to 73·728; or 100 to 135·633.

Whence the mean will be,

100 corresponds to 73·726; or 100 to 135·636.

34. Before quitting this salt, I cannot omit the opportunity of mentioning an instructive circumstance connected with my first experiments. The hydrochloric acid employed was obtained pure (as I then thought) by the redistillation of some ordinary pure acid. It was tested for nitric and sulphuric acids, but there was not a trace of either. Two fluid ounces yielded by evaporation only ·014 of impurities. I performed four operations, and the results were in the ratio of 100 of nitrate of potassa to

73·877 . . 73·888 . . . 73·886 . . . 73·877 of chloride.

The close approximation of these numbers inspired considerable confidence at the time, and in order to verify them, I converted a portion of pure chloride of potassium into nitrate. The result was as 100 of nitrate to 73·728 chloride. The difference between this and the above result indicated some source of error. The chloride, resulting from a nitrate experiment, was therefore reconverted into nitrate by nitric acid, but instead of obtaining the original quantity, I obtained much less. This circumstance suggested, that the error arose from some impurity in the materials, and suspecting the hydrochloric acid, a portion of it was submitted to another distillation. With this new acid another portion of nitre was decomposed, but the result was worse instead of better. The proportion was as 100 of nitrate to 74·04 of chloride. It was evident that some adventitious matter, more volatile than hydrochloric acid, was passing over with the acid in distillation. Sulphurous acid was suspected. A portion of the hydrochloric acid was boiled with a little nitric acid, diluted, and then tested with muriate of baryta; a precipitate of sulphate of baryta deposited. Therefore the acid did contain sulphurous acid, and this, by the action of nitric acid or a nitrate, was converted into sulphuric. The production of a higher result is therefore explained, and although vexatious at the time, it will now show the importance of obtaining the same result from different sources, and how admirably these reciprocal decompositions, of chlorides into nitrates and nitrates into chlorides, are adapted for this purpose.

Chlorate of Soda.

35. This salt has been very imperfectly examined. It is recommended to be prepared by passing chlorine through a solution of soda, and separating the chlorate and chloride by crystallization, or by saturating chloric acid with carbonate of soda. These processes, however, are very uncertain, and this may explain the contradictory descriptions given of this salt in chemical works. The process by which I succeeded in obtaining it pure, is as follows: some bitartrate of soda was prepared from carbonate of soda and tartaric acid, and purified by recrystallization. A quantity of pure chlorate of potassa was dissolved in a sufficient quantity of hot distilled water, and this solution while warm was mixed with a saturated hot solution of the above bitartrate of soda. Instant decomposition occurred; cream of tartar was precipitated, and chlorate of soda remained in solution. The whole was permitted to cool, and when quite cold the solution was decanted, evaporated, and crystallized. It is better to employ excess of chlorate of potassa: the proportions which I used were 32 of chlorate and 40 of bitartrate in crystals. The first crop of chlorate of soda generally contains a little bitartrate, but this may easily be separated by cold water, which dissolves the former and leaves the latter. The salt employed for the present inquiry was dissolved in spirit of wine, and afterwards twice recrystallized from distilled water. It was at last crystallized by spontaneous evaporation. The crystals thus obtained were very large. The diagonals of the largest face of many were not less than ¾ of an inch in length. They were either right square prisms or right rectangular prisms. I have never obtained them of any other shape. They contain no water of crystallization, and remain unchanged by exposure to the air: they fuse at a temperature a little higher than that required by chlorate of potassa; and, like this salt, they always undergo a slight decomposition by fusion.

36. Particular attention is required in concentrating the solutions of this salt; for if the heat be carried too far decomposition will take place. Oxygen in abundance is evolved, and chloride of sodium formed. When this has happened the chlorate is spoilt, for the almost equal solubility of the chlorate and chloride renders it impossible to effect their perfect separation. The criterion by which I judge of the proper strength of a solution, is blowing gently on the surface, and observing whether any crystals are formed; if so the heat may be removed.

37. The process for decomposition is the same as that employed for chlorate of potassa. In drying the resulting chloride of sodium, care must be taken to avoid a loss by decrepitation.

38. I subjoin the results of four experiments with the calculation to one hundred parts.

Chlorate.	Chloride.	Chlorate.	Chloride.
81·816	44·950	100	54·940
75·010	41·199	100	54·925
116·655	64·067	100	54·920
130·705	71·800	100	54·933

The mean will be very nearly 100 of chlorate to 54·930 of chloride.

39. The chlorate was next converted into nitrate by a process similar to that adopted for the analogous salt of potassa. I performed three experiments, and the following are the particulars.

Chlorate.	Nitrate.	Chlorate.	Nitrate.
110·843	88·536	100	79·875
118·290	94·492	100	79·882
89·112	65·198	100	79·890

The mean will be as 100 of chlorate to 79·882 chloride. And comparing this with the experiments on the conversion of chlorate into chloride,

100 parts of chloride will correspond to 145·425 nitrate,
and 100 parts of nitrate will correspond to 68·762 chloride.

Nitrate of Soda

40. Was prepared from pure carbonate of soda and nitric acid. The carbonate was obtained pure by repeated crystallizations of common carbonate. It was obtained in fine large crystals, and 200 grains when dried and fused yielded a colourless mass. This mass was then dissolved in distilled water and tested for chlorides, sulphates, lime, iron, &c., but no trace of impurity could be detected. The crystals were dissolved and saturated with pure nitric acid in excess; and the solution evaporated to dryness. The dry salt after heating to expel the excess of acid, was recrystallized three times. The resulting nitrate was quite pure, and was dried previously to use, by fusion in the platina crucible.

41. Its decomposition into chloride of sodium was effected by the same process as that applied to nitrate potassa. In drying the resulting chloride care must be taken to prevent a loss from decrepitation. Fusion must also be avoided, as the salt rapidly sublimes when fused. The hydrochloric acid was added in two portions, and from one ounce to one ounce and a half was sufficient for the quantities of nitrate employed.

42. I subjoin the results of six experiments with the calculations to 100 parts.

Nitrate of Soda.	Chloride Sodium.		
1. 56·606	38·926	100	68·767
2. 59·220	40·731	100	68·780
3. 50·316	34·602	100	68·770
4. 51·794	35·618	100	68·769
5. 55·250	37·995	100	68·770
6. 68·430	47·059	100	68·770

The mean of these results give the following proportions.

100 parts of nitrate soda correspond to 68·771 chloride sodium,
and 100 parts of chloride sodium correspond to 145·410 nitrate.

Chloride Sodium.

This salt was prepared by supersaturating pure carbonate of soda with pure hydrochloric acid. The solution was evaporated to dryness, and the dry salt, after heating to expel the excess of acid, was redissolved and recrystallized several times. The crystals obtained were quite pure, and were fused in the platina crucible to ensure dryness. It was converted into nitrate of soda by the same method as chloride of potassium. The resulting nitrate was fused in the flask. The following are the results of seven experiments. Number 3. was performed upon some chloride of sodium obtained from chlorate, by heating until all the oxygen was expelled. Number 7. was performed upon chloride in powder, dried in the platina crucible by the flame of a spirit lamp. Half an ounce of the nitric acid (27.) was sufficient for all the following experiments.

	Chloride.	Nitrate.	Chloride.	Nitrate.
1.	43·758	63·630	as 100 to	145·415
2.	50·454	73·465	as 100 to	145·408
3.	38·160	55·492	as 100 to	145·420
4.	48·914	71·112	as 100 to	145·424
5.	56·240	81·778	as 100 to	145·410
6.	58·942	74·021	as 100 to	145·418
7.	99·168	144·210	as 100 to	145·420

whence taking the mean

100 parts of chloride will correspond to 145·416 nitrate,
or 100 parts of nitrate will correspond to 68·768 chloride.

43. The conversion of chloride of sodium into nitrate of soda, affords one of the best and easiest methods for testing the accuracy of the equivalent numbers in general use. According to one series we have 60 as the number for chloride of sodium, and 86 for nitrate; another series gives 58·72, and 85·45 for the same compounds. Now taking these two series as the data of separate calculations, we find, that, according to the former, a hundred grains of chloride should yield 143·333 grains of nitrate; whereas the latter would give 145·504 grains of nitrate from the same quantity of chloride. Here then is a difference of nearly 2·2 grains;—a difference far exceeding any error of manipulation, and plainly indicating that one or other of these series must be incorrect. Upon which series the greater error is chargeable, every chemist may satisfy himself. The decomposition by nitric acid may be readily performed in flasks; and the only requisite for success is purity of materials.

Silver.

44. The object for which the following experiments were made upon this metal has been already stated (5.). The conversion of silver into chloride has been so frequently repeated by the most skilful chemists, that I conceived the better plan would

be to collate their results, and to take the mean as the basis of my calculation. But the discrepancies existing between them, and the objectionable processes which have in many cases been applied, urged me to examine the subject for myself.

45. The purity of the silver employed was very rigorously tested. One hundred and fifty grains were dissolved in pure dilute nitric acid; not a trace of insoluble black matter indicative of gold could be recognised. The nitric solution, being diluted, was precipitated by excess of pure hydrochloric acid; and the supernatant liquor, separated from the precipitate by filtration, was evaporated to dryness. A very minute quantity of solid matter remained; equivalent only to the impurities introduced by the acids and the water. It was examined for copper, lead, and iron, but not the slightest indication of their presence was afforded. The silver was in small pieces, and each piece was sufficient for an experiment.

Conversion into Nitrate.

46. The process for the conversion into nitrate was in all particulars identical to the one described in paragraph number 9. One fluid drachm of the pure nitric acid (27.) diluted with two drachms of distilled water was amply sufficient for the quantities of silver employed. With acid thus diluted the action is very gentle, and, if time be allowed, the whole of the silver may be dissolved without the assistance of heat. During the evaporation of the water and the excess of nitric acid, the receiver flask may be detached without any risk of loss. The dry nitrate was in all cases fused; but particular care is necessary to accomplish this operation successfully, for the salt being in a small cake, is, from the shape of the flask, much thinner at the edges than in the middle; and therefore, unless the heat be cautiously applied, a slight reduction into oxide will happen. This effect becomes evident by the fused nitrate being tinged with black. It only happened in one of the following experiments, and then a very minute black film could be detected on one side of the fused mass. During fusion and cooling, the flasks were always excluded from light.

47. The distilled water employed in these experiments had been twice redistilled in green glass, and four fluid ounces when evaporated left ·024 of a grain of solid matter. The quantity used, namely two drachms, would not introduce therefore a thousandth of a grain of impurity.

48. I subjoin the results of six from nine experiments, with the calculations to 100 parts.

	Silver.	Nitrate.		Silver.	Nitrate.
1.	59·087	93·020	or as 100 to		157·430
2.	60·311	94·952	or as 100 to		157·437
3.	51·654	81·333	or as 100 to		157·458
4.	55·734	86·765	or as 100 to		157·440
5.	45·622	71·827	or as 100 to		157·430
6.	64·726	101·913	or as 100 to		157·455

whence, taking the mean, 100 parts of silver correspond to 157·441 of nitrate.

Conversion into Chloride.

49. The quantity of chloride resulting from a known weight of silver was determined as follows. To each of the fused nitrates from several of the foregoing experiments (48.), half a fluid ounce of distilled water (47.) was added, and the salt dissolved with the assistance of a gentle heat. The solution being slightly acidulated with nitric acid, was precipitated by an excess of pure hydrochloric acid, amounting to half an ounce. These operations were performed without removal from the flask. The precipitated chloride, notwithstanding constant agitation during the addition of the hydrochloric acid, forms into a cake, and lest this should retain any undecomposed nitrate of silver, a glass rod of a dark green colour was introduced and the mass broken up. The rod, on removal from the flask, was always examined with a lens to see whether any chloride had adhered. The flask was next placed upon a heated sand bath, and the water and excess of acid cautiously expelled. The resulting chloride was beautifully white, and was always fused in the flask. During this operation, and while cooling, light was carefully excluded.

50. In the Philosophical Transactions for 1833, the late and much-lamented Dr. Turner has stated that chloride of silver may be dried perfectly and without the risk of decomposition at a temperature of 300° Fahr.; but he adds that even after this a slight loss occurs during fusion. With the latter part of these remarks my observations accord. In several experiments in which the chloride has been dried for a considerable time with a spirit lamp, guarding carefully against fusion, I have found that thus dried it always decreased slightly in weight by fusion, amounting to about ·006 of a grain for 70 grains. Dr. Turner attributed this loss to a slight decomposition. I have repeatedly tried to satisfy myself on this point, but have hitherto been unsuccessful. I have never seen any evidence of decomposition.

51. In five experiments performed as already stated, I obtained the following results.

	Silver.	Chloride.	Silver.	Chloride.
1.	59·087	78·489	or 100 to	132·836
2.	60·311	80·117	or 100 to	132·840
3.	51·654	68·612	or 100 to	132·830
4.	55·734	74·037	or 100 to	132·840
5.	64·726	85·982	or 100 to	132·840

52. Again, in two additional experiments I dissolved the silver in a sufficient quantity of dilute nitric acid, and precipitated at once with hydrochloric acid, without evaporating and procuring the nitrate. These are the particulars of the experiments.

Silver.	Chloride.		
57·882	76·884	100	132·830
51·380	68·252	100	132·838

Taking the mean of the seven experiments, 100 parts of silver will correspond to 132·836 of chloride.

53. The mean of three experiments, described by Dr. TURNER in the Philosophical Transactions for 1829, gives the ratio of 100 of silver to 132·835 of chloride. The following table, extracted from Professor BRANDE's Manual of Chemistry, will show the results of different experimenters upon the same subject.

Dr. THOMPSON	100 of silver to 133·333 of chloride.
ROSE	100 of silver to 133·014 of chloride.
BERZELIUS	100 of silver to 132·736 of chloride.
GAY LUSSAC	100 of silver to 132·890 of chloride.
MARCET and DAVY	100 of silver to 132·450 of chloride.

54. From the five experiments on the chloride, we can also ascertain the ratio of nitrate of silver to chloride. For as the silver employed had been previously converted into nitrate, we can, by comparing the mean result from the nitrates with that from the chloride, obtain the ratio required. Thus every 100 parts of silver correspond to 132·836 of chloride and to 157·441 of nitrate; whence,

100 of chloride will correspond to 118·523 nitrate,
and 100 of nitrate will correspond to 84·372 chloride.

Nitrate Silver.

55. That 100 parts of nitrate to 84·372 of chloride is near the truth, the following experiments on the crystallized nitrate satisfactorily confirm. The nitrate of silver was obtained from pure commercial silver by solution in nitric acid and crystallyzing. After three crystallizations it was obtained in fine bold crystals, and was quite pure. Its perfect dryness was ensured by fusion in a small glass tube. To convert it into chloride, the same process and precautions were adopted as have already been described. The chloride was always fused.

56. I subjoin the results of five experiments.

Nitrate.	Chloride.	Nitrate.	Chloride.
93·452	78·847	100	84·370
115·414	97·395	100	84·388
65·500	55·267	100	84·377
93·034	78·490	100	84·367
108·645	91·664	100	84·370

The mean of these experiments will be

100 of chloride to 118·520 nitrate,
and 100 of nitrate to 84·374 chloride.

57. Dr. TURNER, in the Philosophical Transactions for 1829, gives the details of two experiments upon the conversion of nitrate of silver into chloride. According to one, 100 parts of nitrate correspond to 84·357 of chloride; and to the other, 100 to 84·389. The mean will be 100 to 84·373. The close accordance of the above results

not only proves the correctness of the experiments, but also the purity of the materials.

58. It now remains to determine from the foregoing experimental data, the equivalent numbers of the several compounds described, and of the elementary bodies of which these compounds are constituted. For the convenience of reference, I have arranged the results in a tabular form, and have included the results corresponding to the two series of numbers in general use in this country. I have taken the liberty to name them according to their respective authors.

	Calculated according to Dr. Thompson.	Calculated according to Dr. Turner.	According to the foregoing experiments.
100 parts of chlorate of potassa yield of chloride	61·290	60·838	60·825
100 parts of chlorate of potassa yield of nitrate.....	82·259	82·646	82·500
100 parts of chloride of potassium yield of nitrate....	134·210	135·845	135·636
100 parts of nitrate of potassa yield of chloride.....	74·510	73·613	73·726
100 parts of chlorate of soda yield of chloride	55·555	55·022	54·930
100 parts of chlorate of soda yield of nitrate	79·630	81·942	79·882
100 parts of chloride of sodium yield of nitrate.....	143·333	145·504	145·414
100 parts of nitrate of soda yield of chloride	69·767	68·666	68·771
100 parts of silver yield of nitrate	156·363	157·546	157·441
100 parts of silver yield of chloride.....	133·333	132·796	132·837
100 parts of nitrate of silver yield of chloride.....	85·272	84·290	84·374
100 parts of chloride of silver yield of nitrate.....	117·808	118·637	118·520

59. Of the six elementary substances included in the above compounds, there is only one upon the number of which chemists are agreed. That is oxygen. Upon a scale in which hydrogen is considered as unity, oxygen is stated to be eight; and as this number is generally employed in this country, we may adopt it as the basis of the calculations in the present inquiry. Should it be proved erroneous by future investigations, the numbers, presently deduced, must undergo a proportionate alteration. Remembering, therefore, that I have merely assumed the number eight for oxygen to be correct, we may proceed according to the method already detailed (5.) to ascertain the several equivalents.

60. Chloride of Potassium. According to the preceding table 100 parts of chlorate of potassa correspond to 60·825 of chloride, and therefore the same quantity of chlorate will contain 39·175 oxygen. But in every proportional of chlorate, there are admitted to be six proportionals of oxygen; whence as 39·175 : 60·825 : : 48 : to the equivalent of chloride of potassium. By performing the operation we obtain the number 74·527. Chlorate of potassa will therefore be 74·527 + 48 = 122·527. Nitrate of potassa is determined by the ratio which chloride bears to nitrate, thus according to the table 100 of chloride correspond to 135·636 of nitrate; and therefore as 100 : 135·636 : : 74·527 : 101·087, which will be the equivalent of nitre. Moreover from what has been stated (5.) we learn, that 122·527, the equivalent of chlorate, minus 101·087 the equivalent of nitrate, equals the difference between chlorine, and nitrogen, namely 21·440;—and also that 101·087, minus 74·527 the number for

chloride, is the difference between one proportional of nitrogen plus six of oxygen minus chlorine: namely 26·560.

61. By performing the same method of calculation with the results from the salts of sodium, we have as follows:

Chloride of sodium	58·500
Chlorate of soda	106·500
Nitrate of soda	85·068
Chlorine — nitrogen	21·432
Nitrogen + 6 oxygen — chlorine	26·568

The close accordance of these latter differences with those from the salts of potassium cannot fail, I think, to inspire confidence in the accuracy of the experiments.

62. From these differences we may readily determine the numbers for the salts of silver as well as those for chlorine and nitrogen. Thus, according to the table, 132·837 of chloride of silver correspond to 157·441 of nitrate: the difference being 24·604. But we have ascertained that the difference between a nitrate and a chloride is 26·565, and therefore as 24·604 : 157·441 or 132·837 : : 26·565 : to the equivalent of chloride or of nitrate of silver. Whence the nitrate equals 169·989 and the chloride 143·424; and as 132·837 of chloride contain 100 of silver, therefore the number for silver is 107·970.

63. Nitrogen will be 14·02 and chlorine 35·454. Potassium therefore is 39·073, and sodium 23·046.

64. For the convenience of reference I have subjoined the numbers of these elementary bodies in the following table; with the numbers given by the two distinguished chemists whose names I have had occasion to mention.

	Thompson.	Turner.	Penny.
Oxygen	8	8	8
Chlorine	36	35·42	35·45
Nitrogen	14	14·15	14·02
Potassium	40	39·15	39·08
Sodium	24	23·3	23·05
Silver	110	108·0	107·97

65. These researches corroborate the conclusions to which Dr. Turner was led by his experimental inquiries, published in the Philosophical Transactions for 1833. They show that the estimates in general use among British chemists are not the strict representatives of chemical truth, founded on experiment; and that the favourite hypothesis, of all equivalents being simple multiples of hydrogen, is no longer tenable. My estimates of chlorine and silver correspond very closely to those of Dr. Turner. His number for nitrogen was deduced from experiments on the nitrates of silver, lead, and baryta. He obtained the numbers 14·09, 14·17, 14·2, and he adopted 14·15 as the mean. I have carefully re-examined my experiments, but I cannot discover

anything to justify an alteration of the number 14·02. The experiments upon the conversion of silver into nitrate have satisfied me that 14·15 for nitrogen is inaccurate; for by reference to the table in page 31, it will be seen that according to this number I should have obtained 157·546 of nitrate from 100 of silver, whereas I obtained 157·441. The difference, namely, 0·1, is too high to be referable to errors of manipulation, considering the simplicity of the process employed. Dr. TURNER's numbers for potassium and sodium are not founded upon any experiments of his own, but are extracted from BERZELIUS's Tables of Equivalents.

66. In conclusion, I may mention the several additional subjects which the present inquiry has elicited for investigation. I have extended the decomposition of nitrates into chlorides, and chlorides into nitrates, to the salts of baryta, lime, strontia, manganese, and lead; but whether these salts are applicable to the determination of equivalent numbers, must be decided by future experiments.

67. I have also satisfied myself, that the same method of investigation may be applied to several iodates and iodides, bromates and bromides. They are converted into chlorides by hydrochloric acid; and the iodides and bromides may be converted into nitrates by nitric acid. These decompositions afford excellent means of determining the equivalents of iodine and bromine, and of corroborating the experiments already described upon the nitrates and chlorides. For suppose that two equal quantities of pure iodate or bromate of potassa are converted into chloride and nitrate, it is obvious that the quantities of these resulting salts should bear the same ratio to each other as that established in the present paper. The same ratio should result from the iodides and bromides, if the experiments be correctly performed. Opportunity, however, will not permit me at present to complete the experiments undertaken upon these salts. The special care required in the preparation of the materials, the number of experiments to be performed, and the extreme delicacy necessary to attain truth, would occupy so much time, that I have deemed it better to communicate the results already obtained, than to wait for the completion of the whole. Moreover the details would far exceed the just limits of a single communication.

68. I have likewise examined the action of nitric acid upon chlorates and iodates, and have obtained some novel and interesting results. The process so frequently referred to has also enabled me to make some satisfactory experiments upon the conversion of carbonate of soda into nitrate and chloride, and to obtain some important evidence respecting the equivalent of carbon.

69. As soon as opportunity will permit me to resume and complete these several investigations, I shall do myself the honour of communicating them to the Royal Society.

Apothecaries' Hall,
January 10th, 1839.

III. *Researches on the Chemical Equivalents of Certain Bodies. By* Richard Phillips, *F.R.S., &c.*

Received January 17,—Read February 14, 1839.

The late Dr. Turner, in an elaborate memoir on atomic weights, contained in the Philosophical Transactions for 1833, observes, that "Dr. Prout's hypothesis as advocated by Dr. Thomson, that all atomic weights are simple multiples of that of hydrogen, can no longer be maintained," and he further asserts that hypothesis "to be at variance with the most exact analytic researches which have been conducted."

Although the experiments of Dr. Turner, and the inferences which he has drawn from them, agree very nearly with those of Berzelius, it still appeared desirable further to investigate this subject, and it occurred to me that the inquiry might be conducted in a mode not liable to some of the objections which may be urged against the processses usually adopted.

For the purposes of this investigation, I deem it peculiarly fortunate that Dr. Turner has adopted a whole number as the equivalent of silver, and on this account, as well as for other obvious reasons, I selected it as the basis for an inquiry respecting the equivalents of chlorine and some other elementary gases.

From Dr. Turner's experiments (Philosophical Transactions, 1829), it appears that 108 parts, or one equivalent of silver, yield 143·424 parts of fused chloride, which result coincides very nearly with the determination of Berzelius. Dr. Prout, however, has objected to the fusing of the chloride of silver, that during the operation, it yields hydrochloric acid, which is admitted by Dr. Turner to be the case, although he could not discover that chloride of silver, that had been dried at 300°, lost so much as a thousandth of its weight by subsequent fusion.

It seemed to me that the chance of error arising from the fusing of the chloride of silver might be entirely removed, and other advantages gained, by making an experiment with silver on a large scale, with such proportions of the substances employed as were deemed to be equivalents; and instead of calculating from the whole product of the fused chloride, to do it merely from the weight of such small portion only as might arise from the difference between theoretical views and experimental results.

With this purpose I purified some silver by dissolving it in nitric acid, precipitating by a chloride, dissolving the precipitate in ammonia, again throwing down the chloride and reducing it to the metallic state.

I could not discover any impurity in the silver thus obtained: I therefore dissolved 216 grains (2 equivalents) in nitric acid, and decomposed the nitrate formed, by

adding 108 grains of pure hydrochlorate of ammonia dissolved in water. I need hardly state, that the weight of the hydrochlorate was taken on the assumption, that this salt contains one equivalent of chlorine 36, one of azote 14, and four equivalents of hydrogen 4, equal 54. The chloride of silver precipitated was collected and washed on a filter, but instead of drying and fusing it, it was neglected, for a reason already assigned. Having ascertained that the solution from which this chloride was precipitated contained excess of hydrochlorate of ammonia, I added nitrate of silver to it and to the washings; the chloride of silver thus obtained was collected on a double filter of Chinese paper and washed and dried; it weighed 2·58 grains, a quantity too small to admit of any appreciable error from deficient drying in not subjecting it to fusion. Assuming that 144 of chloride of silver indicate 36 of chlorine, 2·58 grains will give 0·645 grain as the weight of the excess of chlorine in the two presumed equivalents of hydrochlorate of ammonia, and one half of this, or 0·322 grain, subtracted from 36, will reduce the equivalent of chlorine from that number to 35·678. On repeating this experiment I obtained 2·56 grains of chloride of silver, which brings the equivalent of chlorine to 35·680.

In order to bring under discussion the equivalents of other elementary bodies, I prepared some nitrate of silver from the pure metal; it was twice crystallized, and a portion of it, fused in a glass capsule, weighed 271·57 grains; adopting the equivalent weights above stated, and 8 for oxygen, 54 of hydrochlorate of ammonia should decompose 170 of nitrate of silver, and 86·263 of the hydrochlorate would therefore be required for the decomposition of the fused nitrate.

The solutions of these quantities of the salts were accordingly mixed, the precipitate was, as before, separated and washed, and on adding nitrate of silver to the filtered solution and washings, 1·74 grain of chloride of silver was obtained, indicating an excess of 0·435 grain of chlorine; now according to the assumed equivalents, 54 of hydrochlorate of ammonia contains 36 of chlorine, 86·263, the quantity employed in the experiment, therefore contained 57·508, from which, if we subtract 0·435, the excess, there will remain 57·073; if then 86·263 give 57·073, 54, the equivalent of hydrochlorate of ammonia, will contain 35·727 of chlorine instead of 36. This experiment was repeated with 357·34 grains of fused nitrate of silver, and 113·508 grains of hydrochlorate of ammonia; the excess of chlorine was 0·7 grain, which calculating as before, makes the equivalent of chlorine 35·667; and taking the mean results of the four experiments, viz. 35·678, 35·680, 35·727, and 35·667, we have 35·688 as the equivalent of chlorine.

Although Dr. Prout's hypothesis requires that the error, or the difference between 35·688 and 36 = 0·312, should not be divided among the various substances employed in the experiment, it may, nevertheless, be worth while to observe to what the error amounts when the numbers and quantities of the elements included in the operation are summed up: they are

1 equiv. silver . . .	108	
1 equiv. chlorine . .	36,	in the hydrochlorate of ammonia.
4 equivs. hydrogen . .	4,	one in the acid, three in the alkali.
6 equivs. oxygen . .	48,	one with the silver, five in the acid.
2 equivs. azote . . .	28,	one in the acid, one in the alkali.
14 equivs. weighing . .	224	

We have thus 14 equivalents of various elements, the sum of whose weights is 224, yet the mean error, or 0·312, without allowing for circumstances which I shall presently notice, is only about 1–717th part of the whole weight. It may also be remarked, that omitting the silver, and considering the other elements in their gaseous state, the error in volume will be comparatively less than that in weight; though it must at the same time be admitted that this difference is derived from the greater density of chlorine than of the other gases.

1 equiv. chlorine	36 grains =	46 cubic inches, nearly.
4 equivs. hydrogen . . .	4 grains =	186 cubic inches.
6 equivs. oxygen	48 grains =	139 cubic inches.
2 equivs. azote	28 grains =	93 cubic inches.
Total		464 cubic inches.

The weight of chlorine in error being, as already mentioned, 0·312 grain, or about 0·4 cubic inch, is only 1–1160th of the whole volume of the gases.

I have every reason to believe that all the substances which I used in the experiments above detailed, were of the greatest degree of purity; but I may observe that the admixtures most likely to occur in any of them would increase the quantity of the chlorine, or diminish that of the silver; and any substance producing either of these effects would erroneously diminish the equivalent of chlorine by increasing the weight of the precipitated chloride of silver, assumed to be derived from the error of the theory; thus hydrochlorate of ammonia always contains sufficient excess of acid to redden litmus paper, and any moisture or foreign metal which the nitrate of silver might contain would produce corresponding results.

From these experiments and considerations I am of opinion, that no material, and scarcely even any appreciable error can arise from considering the equivalents of hydrogen, oxygen, azote, and chlorine, as 1, 8, 14, and 36 respectively.

The specific gravity of oxygen and azote may be obtained by comparing their equivalent weights with the composition and density of atmospheric air. The mean of various experimental results of the weight of 100 cubic inches of air is about 31 grains, and this agrees very nearly with the determination of Dr. Prout. Now if pure air consist of 20 cubic inches of oxygen and 80 of azote, or of one equivalent of oxygen and 2 equivalents of azote, the weights of which are respectively 8 and 14, the 20 cubic inches of oxygen will weigh 6·88 grains, and the 80 of azote 24·08 grains,

giving 30·96 grains as the weight of 100 cubic inches of air; if, as more commonly admitted, air consists of 21 volumes of oxygen and 79 of azote, we shall have 7·224 grains as the weight of the oxygen, and 23·779 as that of the azote = 31·003 grains as the weight of 100 cubic inches of air; either of these determinations is sufficiently near to show, that we cannot be far wrong in estimating the weight of 100 cubic inches of oxygen at 34·4 grains, and the same volume of azote at 30·1 grains; and the weight of the equivalent of hydrogen being $\frac{1}{8}$ that of oxygen, and its volume twice as great, and the equivalent of chlorine being to that of oxygen as 36 to 8, while their volumes are equal, it follows that 100 cubic inches of hydrogen and chlorine will weigh respectively 2·15 and 77·4 grains, and the density of the four elementary gases in question, compared with air = 1, will be

Hydrogen	0·06935
Azote	0·97097
Oxygen	1·10968
Chlorine	2·49678

On comparing these densities with those stated by Professors Thomson, Turner, and Graham, it will be observed that they are less than those given by Dr. Thomson, which is accounted for by his having assumed that 100 cubic inches of air weigh 31·1446 grains, while they vary, but not very materially, from the weights assumed by Professors Turner and Graham, in being in some cases rather lighter and in others somewhat heavier.

	Thomson.	Turner.	Graham.
Hydrogen	0·0694	0·0690	0·069
Azote	0·9722	0·9727	0·976
Oxygen	1·1111	1·1025	1·1026
Chlorine	2·5000	2·4700	2·470

IV. *Observations on the Parallel Roads of Glen Roy, and of other parts of Lochaber in Scotland, with an attempt to prove that they are of marine origin. By* Charles Darwin, *Esq., M.A. F.R.S. Sec. G.S.*

Received January 17th,—Read February 7th, 1839.

After the two elaborate memoirs which were read nearly at the same time, before the Edinburgh Royal Society and the Geological Society of London, by Sir Thomas Lauder Dick and Dr. Macculloch, on the parallel roads of Glen Roy and the neighbouring valleys, any detailed account of the physical structure of that remarkable district would be superfluous. But from the excellence of these papers and the high authority of their authors, it is necessary carefully to consider the theories they have advanced,—a necessity I feel the more strongly, from having been convinced during the few first days of my examination of the district, that their conclusions were impregnable. Moreover the results to which I have arrived, if proved, are of so much greater geological importance than the mere explaining the origin of the *roads*, that I must beg to be permitted to enter into the subject in detail.

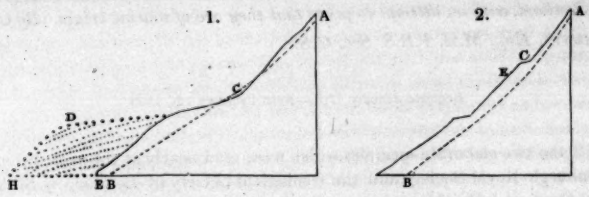
Section I.—*Description of the Shelves.*

The parallel roads, shelves, or lines, as they have been indifferently called, are most plainly developed in Glen Roy. They extend in lines, absolutely horizontal, along the steep grassy sides of the mountains, which are covered with a mantle, unusually thick, of slightly argillaceous alluvium. They consist of narrow terraces, which, however, are never quite flat like artificial ones, but gently slope towards the valley, with an average breadth of about sixty feet. There are only four shelves which are *plainly* marked for any considerable length; the lowest one according to Macculloch is 972 feet above the sea; the next above it is 212 feet higher, and the third, eighty-two above the second, or 1266* above the sea; the fourth occurs only in Glen Gluoy; it is twelve feet higher than the third. I shall refer to them either by their absolute altitude, or as being the upper or lower one in the part under description, and not as first, second, or third; for it will be hereafter seen that others occur in every respect similar, only less plainly developed.

It is admitted by every one, that no other cause, except water acting for some period on the steep side of the mountains, could have traced these lines over an exten-

* Some rude measurements which I made with a mountain barometer lead me to suspect that these altitudes are at least a hundred feet too great. It is not a point of any importance with respect to the theory of the origin of the shelves, but I regret that I did not verify their height with more care.

sive district. The dark line in the accompanying wood-cut (No. 1.) represents the real profile of a shelf, and is copied from MACCULLOCH. To this I have added two imaginary lines, of which the broken one gives the supposed original form of the



A B supposed original surface of rock; C E line of shelf; C D H line of shelf when expanded into a buttress or terrace.

underlying rock. The formation of the shelf, as may be here seen, is chiefly due to the accumulation of matter in the form of a mound, only very slightly projecting beyond the general slope of the mountain, and partly to the removal or corrosion of the solid rock. The latter effect, although well marked in some particular spots, cannot generally be distinguished, and the shelves no doubt are chiefly due to the accumulation, and not to the removal of matter. In this same diagram (1.) the covering of alluvium is represented as thicker some way below the shelf, than at the same distance above it. I believe this is generally the case, and hence the projection of the shelf is often very obscure; and when two or three occur, one below the other, their outline closely approaches to that represented in wood-cut (2.). MACCULLOCH will scarcely even allow that a shelf in any case forms a projecting mound; but this certainly is incorrect, and is indeed contradicted by his own statements, and by that implied in the comparison of the shelves with the beaches of lakes, which have been suddenly drained. The shelves entirely disappear, where crossing any part of the mountains in which the bare rock is exposed; for loose matter cannot accumulate there, and the rocks themselves from their laminated structure do not readily become worn into any regular form. They likewise disappear where crossing any part which is gently inclined; for their own slope then coincides with that of the alluvial covering, and cannot be distinguished from it.

The *dotted* line in the wood-cut (No. 1.) is supposed to represent the broader terraces, or even plains, of stratified shingle, sand, and mud, with which the shelves often become united. These terraces do not differ from the shelves in any one essential point of structure, but are much broader; and as the matter of which they are composed is in much larger quantity, a rude kind of stratification may be generally observed. They occur only where the bottom of the valley in its gradual ascent rises nearly to the level of a shelf, or at points on the hill-sides, where it is probable that the streamlets formerly brought down much detritus to the ancient beaches.

Sir LAUDER DICK has observed* that the shelf infallibly intersects the head of such terraces or buttresses: this certainly is the case (as in diagram 1.) with all the smaller ones; and, therefore, we may infer that their formation dates from the period when the shelf was a beach. But at the head of the greater valleys, where the supply of matter must have been more abundant, and where the slope of the land was highly favourable to its accumulation, the line of shelf sweeps across and blends into a plain, which has an uniform slope upwards and downwards above and below that level. Therefore, when the water stood at any one of the shelves, there were many little deltas which did not rise above its level, but some greater ones that were continuous with an upward slope of shingle, filling the bottoms of the main valleys†.

The shelves are chiefly composed of the same kind of alluvium with that covering the whole surface of the mountain; and they seem to have been formed, as suggested by MACCULLOCH, by the check given to the downward descent of ordinary detritus, and that transported by torrents, at the level of the ancient waters; I could perceive no difference in the nature of the alluvium above and below the upper shelf, as stated to be the case by Sir LAUDER‡. It contains fewer well-rounded pebbles at the greater heights than would have been expected on any theory of the origin of the shelves; but they are abundant in the lower and broader parts of the valleys. Nevertheless where there is any level spot at the height of the upper shelves, well-rounded pebbles may generally be found, as on the summit of a rounded hill, or a flat little strait separating some hillock from a line of shelf (for instance near Craigdhu, on the summit of Meal Roy, and between Upper and Lower Glen Roy). In these cases the pebbles must have been almost exclusively formed by the action of the currents and waves of the former expanse of water. They are frequently derived from rocks not found in the immediate vicinity: erratic boulders also are scattered over these mountains. I state these facts distinctly, because MACCULLOCH says§ that the composition of the alluvium of the upper shelves is *wholly* different from that covering the sides of the broad valleys; whereas the difference is only one of degree, for which many causes might be assigned.

I have already observed, that the quantity of solid rock worn away on the line of any shelf is not usually great. At the narrow entrance, however, of Loch Treig (of which a drawing is given by Sir LAUDER DICK), on the west side, which is very steep, the gneiss is worn into smooth concave hollows, the peculiar curves of which, though

* Transactions of the Edinburgh Royal Society, vol. ix. p. 11.

† These statements are founded on what I saw in Glen Collarig, where the lower shelf (the 972 feet one) blends into a slope, now rendered irregular by the action of the torrents, which rises (at the gap) to a height of more than a hundred feet above the level of the shelf. Again, near the head of Lower Glen Roy, the seam shelf blends into a similar kind of plain, which rises (at the base of a terrace, projecting from the next shelf to it,) ninety feet (barometrically measured) above the level of that shelf to which it may be said to belong. In the east arm of Glen Turet, the upper shelves in a like manner terminate in slopes, which rise above their proper levels.

‡ Geological Transactions, vol. iv. (First Series), p. 320–338, and 387.

§ Edinburgh Royal Transactions, vol. ix. p. 12.

they cannot be described, may be readily imagined by calling to mind the form of rocks washed by a water-fall. This was the only one spot where I could observe this appearance in an unequivocal manner; but this one point of rock would to my mind carry demonstration with it, even if there were not innumerable other proofs, that the water had remained at the level of the 972 feet shelf for a very long period*. On the opposite side of the entrance, or gorge, which here slightly bends before entering Loch Treig, the shelf expands into a line of terrace. Standing on the precipitous and waterworn rocks, it required little imagination to go back to former ages, and to behold the water eddying and splashing against the steep rocks on one side of the channel, whilst on the other it was flowing quietly over a shelving spit of sand and gravel. The only other and rather different case of waterworn rock, which I noticed, was at the head of Lower Glen Roy (pointed out by Sir LAUDER DICK), where the summits of some irregular hummocks of gneiss on a level with the upper shelf were obliquely truncated by a smooth surface. I have frequently observed a similar structure on the rocky shores of protected harbours. Large fragments of rock are scattered on most of the shelves, of which many are of granite, and have come from a distance, as will presently be described; the greater number, however, have merely rolled down from the heights above. Of the latter, some have fallen recently, whilst others are waterworn, as if they had lain for centuries on a sea coast; and it was in many cases easy to point out, whilst walking along the level shelf, which fragments had been washed by the ancient waves, and which had fallen since.

Sir LAUDER DICK has observed, and the fact is very important, that the head of Glen Gluoy is separated from the head of a branch of Glen Roy by a flat land-strait, with which the shelf in the former glen is exactly on a level; so that if Glen Gluoy were filled with water to the *full* level of its shelf, or a few inches above it, besides a great barrier at the lower end, a little mound, perhaps a foot or two in height, would be required to prevent the water flowing into Glen Roy. In the same manner if Glen Roy were closed at its lower end, and if water stood at the level of the upper shelf, it would trickle into the valley of the Spey. The same thing would happen with the lower shelf at the head of the valley of the Spean; and lastly, a short shelf, which I discovered in a gully, which enters the Caledonian Canal near Kilfinnin†, between

* After the elaborate arguments given by MACCULLOCH, to show that no sudden rush of water, or debacle, could have formed the shelves, I should not have offered any remarks on this point, had not so distinguished a person as Sir GEORGE MACKENZIE (London and Edinburgh Philosophical Magazine, December 1835,) suggested such an hypothesis, without, however, it is fair to add, having visited the district. Each of the ten thousand pebbles, which together form any one buttress or little delta, and which, it is evident, were accumulated by the action of one streamlet, at the spot where it entered the expanse of ancient water,—each of these pebbles required time for its attrition,—each now plainly speaks against such an hypothesis.

† I was informed, but whether correctly I do not know, that the hamlet (in the middle of which there is a mound with a round tower on it,) on the opposite side of the valley, and a mile or two south of Invergarry, was named Kilfinnin. I therefore shall denominate the small stream which flows towards the Caledonian Canal at that point by this name. In the same manner I shall call the larger stream which debouches by Habercalder, and its valley, by that name, not having been able to learn any more proper one.

Loch Oich and Loch Lochy, is in a similar manner on a line with a peat moss, forming the watershed between it and another small valley. These four cases are so remarkable, that the coincidence of level must be intimately connected with the origin of the shelves; although such relation is not absolutely necessary, in as much as the middle shelf of Glen Roy is not on a level with any watershed. Sir LAUDER endeavours to explain this fact by supposing that when the imaginary barriers of his separate lakes were perfect, the water flowed from that end of the glen, which is now highest, in other words, that the drainage of the supposed lakes was in each case in a reverse direction to that of the streams now occupying their beds. This view implies, moreover, the strange accident, that, during the breaking down of the barriers, the part that was originally lowest always remained standing, whilst a higher part gave way; and thus the removal of the barrier must be supposed to have happened from the effects of some causes no ways analogous to the wearing down of the mouths of lakes as they ordinarily exist.

The structure of these land-straits must be now described. This has already been minutely done by Sir LAUDER with respect to that one which connects the sources of Glen Gluoy with those of Glen Turet, one of the arms of Glen Roy. The only additional observation which I have to make, is that the strait is broad and very level, and that on one side I noticed a beach, like that on a sea shore, of well-rounded pebbles. The accounts given by MACCULLOCH and Sir LAUDER of the division of the waters of Glen Roy and the Spey differ in some essential points. The latter author states that the upper shelf of Glen Roy is on a level (excluding the peat-moss) with the flat where the waters divide. This appears to be accurately the case, as far as the mountain barometer (which stood at the same thousandth of an inch on the two stations) and my eye could be trusted. But on the north side of the watershed there are patches of little terraces about fifteen feet above this level, resembling those which in other parts are connected with shelves, and hence probably having a similar origin with them. On the hill-side higher up, other obscure patches of alluvium occur with somewhat analogous forms. The water of the Spey first flows down a gentle mossy slope eastward, and is then collected in Loch Spey. On the south side of this loch, there is an obscure line of terrace, which appears to be about sixty feet above the loch, and which doubtless led MACCULLOCH to suppose the upper shelf of Glen Roy was that number of feet above the division of water. The terrace above Loch Spey, as far as I could judge by the eye without a levelling instrument, is horizontal, and may perhaps be traced along the south side of the watershed, even a short distance within Upper Glen Roy, where certainly there occurs a mound parallel to and above the upper shelf. I much regret I was unavoidably prevented from examining this locality with all the attention it deserved. But from the structure of the small terraces, it appeared to me certain that water must for a period have occupied a level above that of the highest shelf of Glen Roy; and likewise that fragments of a shelf, or line of terrace, which as far as the eye could judge was horizontal, extended within

the basin of the Spey, and therefore beyond the limits of the supposed lake of Roy. This latter fact is, at least, certain, for I have since learned, through the kindness of Sir DAVID BREWSTER, that he has seen, as will be hereafter mentioned, shelves resembling those of Glen Roy at two points, at a distance of several miles down the valley of the Spey. The watershed at the head of the valley of Kilfinnin, has precisely the same character with the foregoing cases: here also a flat-topped buttress projects on one side above the level of the shelf, and this seems to indicate, as in the former case, the presence of water at a level rather above that of the shelf itself*.

The division of the waters between most of the glens and ravines in this district, in situations where no shelves occur, does not take place on a sharp ridge, but on level, and often broad land-straits, similar to those just described. I may instance a long one (at an elevation of between 1400 and 1500 feet above the sea,) separating two branches of the water which flows by Habercalder into the Great Glen, and one branch of the Tarf Water. Again another one nearer Fort Augustus, separating the two lowest and nearest branches of the same two rivers; here also there were obscure buttresses on each side above the level of the watershed. An intelligent shepherd who accompanied me, remarked that this form of the land was common wherever the waters in this mountainous country divided; and I observed several instances of it. Finally, I may remark, without wishing to lay any great stress on the argument, that these *land-straits*, whether connected with the shelves, or not, are precisely what might be expected from *straits*, properly so called, between arms of the sea being laid dry.

The discovery of any shelf, beyond the limits where they had been hitherto observed, being evidently an important point with regard to the theory of their origin, I shall fully describe the following case. At the head of a small stream which joins the Caledonian Canal, near Kilfinnin, and which is divided from the waters of the Habercalder by a flat mossy watershed already alluded to, some fragments of a shelf occur on the northern side. This shelf resembles in every respect those in Glen Roy; it seemed, as I walked along it, perfectly level, as it likewise did, when I viewed it from either end, and when I crossed the valley. I then took several measurements at the most distant points with the mountain barometer, and the mercury stood within the same hundredth of an inch. On the northern side of the valley, the shelf, which commences on a level with the mossy plain dividing the waters, extends for about a quarter of a mile almost continuously; it is then lost from the number of fragments of rock which have fallen down the hill, but reappears at the distance of more than half a mile from its commencement under the form of two or three little buttresses. These I ascertained by the barometer to be on a perfect level with the commencement of the shelf, or the watershed, a circumstance which was also apparent by the eye alone. The line further on disappears from the rockiness of the sides of the valley.

* The pass of Muckul, described by Sir LAUDER, which separates the waters of the Spean from a branch of the Spey, I did not visit.

On the south and opposite side of the valley, a broad sloping terrace extends at a corresponding level for about three quarters of a mile, but is indistinct owing to the gentle slope of the mountain. Further on it seems modelled into more than one terrace: and these, though obscure, appear to a person standing on them perfectly horizontal. Although the terraces are not plainly developed on this side, yet it is certain, that horizontal mounds, at nearly the level of the watershed, extend about two miles on the face of the mountain. With respect to the absolute elevation of this shelf, I made it about forty feet above the upper one of Glen Roy, and 1120 above Loch Lochy, or 1202 above the sea: but my barometrical observations have no pretensions to accuracy. After having observed this shelf from so many points of view, I am prepared positively to assert that it is in every respect as characteristic a shelf as any in Glen Roy; and although the fragments of it do not extend over more than, perhaps, half a mile in length, its origin must be as carefully attended to in any general theory of the formation of the shelves, as if its length had been twenty times as great. Its want of continuity and shortness possess, indeed, in themselves much interest, because we thus know that those causes which have marked with horizontal lines the sides of the mountains of Glen Roy in so wonderful a manner, have been in action here, though they have produced but little effect. Moreover, we see that if the surface had been originally rather more rocky, or had been less steeply inclined than at present, or had been subjected to a very little more alluvial action, all evidence would have been obliterated of the extension thus far of the action of these causes.

I have already alluded to the important fact communicated to me by Sir David Brewster, namely, that he has seen shelves in the valley of the Spey. At Phones, which is situated about a mile from the Truim, and about five above its confluence with the Spey, one broad and well-marked shelf occurs, along which a carriage can be driven. On the banks of the Spey, about twenty-five miles below its source, two shelves occur in an elevated angle between the Burns of Belleville and the river. They are small; the upper one, however, is very broad; and their elevation is about 800 feet above the sea. Sir David Brewster says, that the shelves in both places appear horizontal, and that they resemble those of Glen Roy, though possessing far less grandeur and symmetry. The fact of their occurrence at these distant points is, as we shall hereafter see, highly important.

Section II.—*The Theories of Sir* Lauder Dick *and Dr.* Macculloch *considered.*

Sir Lauder believes that a separate lake existed in each valley, where we now see a shelf, and was separately drained. In Glen Roy, where three shelves occur, all plainly developed, (with the exception of Belleville, this is the only place where more than one has been observed,) the arguments in favour of a separate lake possess the greatest force. Without entering into any description of the physical features of Glen Roy, inspection of the accompanying map, taken with some few alterations

from that of Sir LAUDER DICK, will show the course of the shelves; although they cannot of course be followed nearly so continuously in nature as here represented. The lower one (972 feet above the sea) is common to nearly the whole line of the Spean and Glen Roy. The two upper shelves are confined to Glen Roy, with the exception of those short portions extending into Glen Collarig. It will be seen that both these lines, if continued round the hill of Bohuntine (at the eastern entrance of Glen Roy), would insulate it, whilst the lower shelf only forms it into a peninsula. From this structure it will be evident, that in order to form Glen Roy into a lake at either of the two upper levels, it would be necessary to erect two barriers, one across Glen Collarig, and the other principal one across the mouth of the Roy.

The lines are here represented as if abruptly cut off, but this is not so; and the following remark holds good in other cases, namely, that where a shelf terminates without any visible change in the nature of the slope, such as being rocky, &c., its disappearance is so extremely gradual, that it can be traced, sometimes to a further and sometimes to a lesser distance, according to the point from which it is viewed. Of this fact the shelves on the south-east side of Glen Collarig offer an excellent example. In the map, the extremities of the lower of the two upper shelves are represented at the four places where they terminate, as extending beyond those of the upper one. I state this on the authority of Sir LAUDER DICK with respect to those in Glen Roy, and it is conspicuously the case with that pair in Glen Collarig which I have described as disappearing in so insensible a manner. The lower line can there be traced, though faintly, to a point below the houses of the glen opposite a small tributary torrent, and therefore considerably beyond (or nearer the mouth) than the point where the 972 feet shelf crosses the bottom of the valley. Observing in Glen Collarig the gradual disappearance of either set of lines, and that there is not the smallest apparent cause for it in the nature of the ground, the first and obvious supposition is that a sheet of water extended from the Spean into Glen Roy and Collarig, and that the mere widening of the mouths of the latter, as they approached the less protected expanse of the Spean, gradually became unfavourable to the accumulation of detritus, and therefore to the formation of the shelves. This view is greatly strengthened by the extension of the lower line in each case beyond the upper; for of course the supposed unfavourable condition for their formation, that is, the too great breadth and exposure of the sheet of water of which they formed the beach, would affect the line when the water stood at the higher level to a greater distance from the main expanse, or further up the valley, than when it occupied a lower level. It may, however, be argued (and on the hypothesis of Glen Roy having existed as a lake it must be so argued), that as the higher line is the oldest, so its terminal portion may soonest have yielded to those causes which modify the surface of the land. This view, however, receives little support from an examination of the rest of the glen, inasmuch as the two shelves through its whole course are in a state of equal preservation. We must therefore conclude, either that we now behold the shelves

precisely as they were left by the sheet of water, or that if the two upper shelves did originally extend for an equal length on each side of the two glens, that the causes which tend in a small degree (for the existence of the shelves proves that no great changes have taken place) to smooth the surface, have acted over this district with the most *perfect uniformity*. Moreover, it may be remarked, that wherever a streamlet crosses a shelf, and it is probable from its size that it formerly delivered detritus to the ancient expanse of water, either a greater breadth of shelf or a small buttress there, attests that it was so; and in doing this, likewise attests how perfectly the surface of the land has been preserved. Now I paid particular attention to the following observation, namely, that on both sides of the hill of Bohuntine, and on the opposed mountains, where the shelves terminate, there was not the smallest change in the composition or in the outline of the smooth rounded surfaces. Yet it is in this very spot, where the lines insensibly disappear,—on these very hills, where the little deltas of the ancient streamlets are still preserved,—within this very district, where in the extension of the lower shelf beyond the upper one in the four cases, we have the most satisfactory proof of the action of absolutely uniform causes, either in their formation or in their obliteration; it is here, where the slope of the turf-covered hills is unbroken, where there is not a remnant of any projecting mass, that we are compelled by the theory to believe that the two enormous barriers stood, which formed Glen Roy into the imaginary Loch Roy.

But as it is highly important to show that such a Loch could not have existed, we must for a time, in the face of these great difficulties, suppose the two barriers to have been erected. It may be first remarked, that from the extension of the middle shelf, the barrier in Glen Collarig could not have occupied the only one place, which the structure of the ground indicates, even in the smallest degree, as probable, namely, at the Gap, where the waters divide; but it is necessary to suppose that it crossed the glen at a point some way distant from the Gap, and where the valley has a depth, below the upper shelf, of more than 300 feet. Glen Roy being now converted into a lake, with its drainage reversed, that is, with the water flowing from it by the Spey to the east coast of Scotland, let one of the two barriers, we will say the smaller one in Glen Collarig, give way from the effects of an earthquake or other cause. The lake will now stand at the level of the middle shelf, the barrier having given way eighty-two feet vertically. Again let it burst, and this time rather more than 212 feet vertical must be swept away, so that the larger lake, supposed by Sir LAUDER's hypothesis to occupy the valley of the Spean at the level of the 972 feet shelf, might send an arm a little way up the glen (as shown by the shelf now existing there) above the point where the barrier stood. Let all this have taken place, but still a barrier nearly a mile long, and 800 feet in height, is left standing across the mouth of the Roy. Must we suppose that each time the barrier in Glen Collarig failed, the one in Glen Roy gave way *the same number of feet* through some strange coincidence? or are we to conclude that some awful catastrophe at sub-

sequent times, unconnected with the drainage of the lake, which must have passed through the breach already opened, removed the second barrier (either part or all of it) when *above water*, without having left the smallest remnant of it, or having disturbed the smooth alluvial covering of the steep slopes? The 972-feet shelf is common to the valley of the Spean and Glen Roy, and is supposed to have been formed by a lake, the barrier of which, some miles in length, extended near Highbridge across the mouth of the Spean. This shelf passes uninterruptedly, and with its usual breadth, on both sides of Glen Roy and of Glen Collarig, in the very part where the barriers of Loch Roy, if they existed, *must* have crossed the valley; therefore the whole, or part of the great base of those enormous barriers, must have been swept away when submerged within the bosom of the imaginary Loch Spean; and this must have been so perfectly effected, that no trace of them is left on the smooth slope of the hill, not even by a greater breadth of the shelf, any more than in the part of the second barrier, which must have been removed when above water*. And all this is supposed to have taken place on the hills, where I have shown how wonderfully the features of the land have been preserved, and where the boulders which were washed by the waves of the ancient water can be distinguished from those which have fallen since. In conclusion, therefore, I do not hesitate to affirm, that more convincing proofs of the non-existence of the imaginary Loch Roy could scarcely have been invented, with full play given to the imagination, than those which are marked in legible characters on the face of these hills†.

The same reasons which render the existence of a separate lake in Glen Roy so excessively improbable, apply with only little less force to each of the imaginary lakes in the other glens. We are, therefore, in giving up *Loch* Roy, involuntarily driven to the theory advanced by Macculloch, namely, that all the valleys in which shelves occur were included in one large lake; but we shall thus run headlong even into greater difficulties. First, from the structure of the mountains, four immense barriers are required to form the lake‡, namely, one low down across the valley of the

* I have not thought it worth while to enter into all the possible cases of this hypothesis, but have merely taken the most obvious one, which was assumed by Sir Lauder. If any one has the boldness to come forward from the obscurity of past times, and state his belief that the broad barrier of the Spean was *erected* as well as removed altogether subsequently to the removal of the two barriers of Glen Roy, then the objection from the uniform breadth of the 972-feet shelf, where crossing the spot which must have been occupied by the barrier of Loch Roy, has less weight, but the other part of the argument remains valid. Again, on the hypothesis in the text, I have not entered into all the possible alternatives of the manner in which the bases of the Loch Roy barriers might have been removed, either when Loch Roy itself, or when Loch Spean was drained, or at some subsequent period by unknown causes connected with the drainage of the imaginary lakes.

† It should be remembered that it is far easier to assert than to disprove. If to explain some phenomenon it as stated that the Thames near London was formerly crossed by a barrier some hundred feet in height, of which it was not pretended a vestige now remained, it is difficult to imagine what kind of evidence would be sufficient to prove the hypothesis false, as long as any one was found willing to admit such an assumption.

‡ I may add, the same number of barriers are requisite, whether we suppose the existence of one, two, three,

Spey, two at distant points across the Great Glen of Scotland, and a fourth across the mouth of Loch Eil, the last being necessary, as MACCULLOCH shows*, from the structure of the Great Glen in that part. It may be safely asserted that more improbable situations could hardly be imagined in the whole of Scotland. It is perhaps useless to ask, were the barriers composed of rock or alluvium? if of the former, they were transverse to every line of hill in this part of the country; if of alluvium, we must assume an unexampled case; for where in the whole world shall we find even one barrier a mile and upward in length, and 1200 feet high, composed of loose waterworn materials? Secondly, the theory of one large lake does not explain in a satisfactory manner the remarkable coincidence between the shelves and the watersheds. Thirdly, when by the bursting of any one of the barriers, the level of the lake had fallen from one shelf to another, the hypothesis requires (as with *Loch* Roy) that the three other barriers, now high and dry, and distant many leagues from each other, should have been swept away by some unknown power, acting by some unknown and scarcely conceivable means, from the smooth sides of the mountains, without a remnant of them having been left; so that MACCULLOCH even frankly confesses one part is almost as probable (I would say improbable) as another for the position of the barriers. And it should be borne in mind, that these extraordinary forces are supposed to have acted on the outskirts of that large area, throughout which we have proofs, most wonderful and unequivocal, of the entire preservation of the surface of the land, as it was left at a period long anterior to the removal (if such removal ever did take place) of the barriers of the lower lakes. I do not hesitate to assert that this one difficulty, even by itself, would be sufficient to refute the theory of one great lake: Sir LAUDER's theory has been shown to be equally untenable. It is perhaps here almost superfluous to add, that the discovery of the shelf at Kilfinnin (and probably likewise of those in the valley of the Spey) increases every difficulty manifold; for the valley of Kilfinnin is almost as wide as it is long, which affects one theory, as the lowness of the opposite side of the Great Glen does equally the other. Finally, then, in giving up both, the conclusion is inevitable, that no hypothesis founded on the supposed existence of a sheet of water confined by *barriers*, that is, a lake, can be admitted as solving the problematical origin of the "parallel roads of Lochaber."

Section III.—*Proofs of the retreat of a body of water from the central parts of Scotland, and that this water was that of the sea.*

Having now discussed these views which cannot be admitted,—a method of reasoning always most unsatisfactory, but necessary in this instance from the high authority of those who have advanced them,—I will consider some other appearances,

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or as many lakes as glens; and the argument against MACCULLOCH's hypothesis of one lake, and against that of the separate lakes by Sir LAUDER, are applicable to any hypothesis requiring an intermediate number.

* Geological Transactions, vol. iv. p. 378.

which will perhaps throw light on the origin of the shelves. The valley of the Spean, from the point where it joins the Great Glen of Scotland to where it receives the Roy, is broad, and its bottom moderately level. The solid rock is concealed in almost every part, excepting where the river has cut itself a gorge, by irregularly horizontal strata of gravel, sand, and mud. Large portions of these beds have been removed along the centre of the valley, yet it is quite evident from the fringe or line of terraces which skirt each side, that the bottom must originally have formed a smooth concave surface inclined towards the mouth of the valley. Portions more or less perfect of this same deposit can be followed up the course of the Roy, and up the higher parts of the Spean, where the valley is not too rocky or narrow, to near Loch Laggan. This loch is but little below the 972-feet shelf; and at present I wish, for the sake of the independence of the argument derived from the facts to be stated, to consider only that part of the country which is below the level of that shelf. These irregularly stratified beds, near the mouth of the Spean, attain a thickness of several hundred feet, and they consist of sand and pebbles, many of the latter being perfectly waterworn. Higher up the valley, near the bridge of Roy, the thickness before the central portions were removed appears to have been about sixty feet, but of course the thickness varies according to the original irregularities of the rocky bottom of the valley. Now it may be asked by what agency has this sloping sheet of waterworn materials been deposited along the course of the valley? From the presence of the horizontal shelves we know that there has been no change in the relative level or inclination of the country since this district was last covered with water, and therefore we may argue with safety, that the action of the rivers, as far as it is determined by their inclination, must have been the same since that period as it now is, with the exception of that amount of change which they may have effected in their own beds. Our knowledge that there has been here no axis of elevation, with one part always rising a foot, and another a few inches less; but that the entire system of drainage has remained undisturbed and subject only to its own laws of change, is a circumstance which gives a singular degree of interest to the examination of this district. Now if we look at any portion of these rivers, for instance the Roy above its junction with the Spean, we find it has cut a narrow steep-sided gorge through the solid rock, which is in many parts between twenty and thirty feet deep, whilst on each side there are remnants, as above stated, of a continuous bed of gravel, at least sixty feet in thickness. These beds have certainly been deposited by rapid currents of water, but not by any overwhelming debacle, as may be inferred from the presence of cross layers, and the alternate ones of fine and coarse matter. Seeing also the evident relation of dimension and materials which exists between these deposits and the valleys in which they occur, it can scarcely be doubted that the detritus of which they are composed was transported by the existing rivers. But are we to suppose that the river, as in the case of the Roy, first deposited along its whole course these layers one over another, thus raising its bed sixty feet above the solid rock, and then

suddenly commenced, without the smallest change in the inclination of the country, not only to remove the matter before deposited, but when having gained its former level, to act in a directly opposite manner, and to cut a deep channel in the living rock? Assuredly such a supposition will not be received; and whatever part the river had in the accumulation of these waterworn materials, from the very moment (neglecting the annual oscillations of action from the changing seasons) it ceased to add and began to remove, its power must have undergone some most important modification.

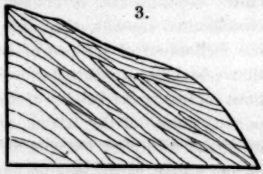
It will perhaps be thought that the mere deepening of the bed of the stream, near the mouth of the valley (the effect being slowly propagated upwards), could have caused the difference between the present and the former action of the river. But it is not difficult to replace in imagination the solid rock in the course of the Spean; and although a few small lakes will be thus formed, the average slope will not differ greatly from the present inclination, and this inclination we see is sufficient to cause the river to wear a deep gorge in the solid rock, and therefore it is evident (although I am aware that without actual measurement of the inclination this argument must rest upon eyesight, which cannot generally be trusted) that a change of this nature would be wholly insufficient to reverse the action of the river, as has here been the case. We must not, of course, at the same time replace in imagination those unconsolidated deposits, the origin of which we are considering; otherwise no doubt the inclination of the bed of the river would be greatly altered; although even in that case I by no means believe that the river would be so much retarded as to deposit matter at the heights where it is now left. Some check, therefore, to the transporting power of the stream seems to have acted at many, or at every successive level. If we reflect on what would result, as an hypothesis, from a river delivering during a long period detritus into a lake, the level of which was gradually sinking from the wearing down of its mouth, a gently sloping surface would be formed at its head. But as the barrier was cut deeper and deeper, and the lake sank, the stream in the part where it was once checked by meeting with the still water would gain velocity, and hence would cut through the beds which it had originally deposited. The fringe, of rudely stratified alluvium, the origin of which we are considering, resembles both in structure and composition such beds of detritus as would have accumulated on the shores of a lake, had one existed in these valleys. If, then, we suppose that a subsiding sheet of water did actually fill this valley, either of one or more lakes, with their barrier gradually wearing down, or of an arm of the sea, the general level of the ocean being stationary during a slow elevation of the land (as now is the case with the fiords of Scandinavia), every appearance on the sides of the valley of the Spean and Roy will be explained; and as there is no other way, that I can see, of accounting for them, the hypothesis is so far worthy of admission.

I ought, perhaps, to have previously observed, that these deposits could not have been formed when the valley was filled with water to the level of the shelves, for the

detritus has the character of matter accumulated in shoal water, and the beds abut abruptly against the bases of the mountains, instead of blending with the alluvium on their surface, as would necessarily have happened had the whole been deposited at the same time at the bottom of one basin.

The conclusion, that these valleys have been occupied by a sheet of subsiding water, follows more plainly from a somewhat different class of facts. I have before remarked, that where a streamlet crosses a shelf, especially if it be the lower one, an obliquely truncated buttress, the form of which was represented by dotted lines in the wood-cut No. 1, projects from the side of the hill. It is quite evident that these were accumulated when the shelf existed as a beach, and the streamlet at present only acts in removing those portions with which it comes in contact. Now in some points where the buttresses have been somewhat largely developed, smaller ones at a lower level, composed of the same irregularly stratified waterworn materials, having nearly the same outline, although unconnected with any shelf, may be observed adhering to the slope of the hill. Instances of this structure occur on the east side of Glen Roy; on the south side of the Spean, and between Loch Treig and the bridge of Roy, the accumulation of perfectly rounded shingle, like that on a sea-beach, was enormous. The internal structure in this instance corresponded to the external form, as is shown in the accompanying diagram, where highly inclined beds of sand and coarse gravel are capped by other irregular ones of the same composition, only slightly inclined. In all these cases, where the flat-topped buttresses occur on steep slopes, it is certain (as might have been expected) that the streamlet is steadily at work in removing matter, and does not add one pebble to the mound. No one will dispute, that those buttresses, which are mere extensions of a line of shelf, were formed at the edge of an expanse of water (of which the shelf was the beach), and it is therefore by itself probable that the other buttresses, of similar external form and composition, though occurring at a different level, had a similar origin. But the argument may be put in a stronger point of view: taking the course of one of these streamlets, and observing the size and position relative to it of the buttresses one above the other, it becomes evident that the materials of which they are formed were accumulated through the agency of this stream, although it is at the same time inconceivable that they were left (especially in such a case as that represented in diagram 3.) on the steep slope by a power which, as it now acts, is steadily at work, tearing away matter in its whole downward course. Therefore, it is absolutely necessary to bring into play some intervening or modifying cause in the action of the streamlet; in the case of the buttresses which are connected with the shelves, no one can doubt what this intervening cause has been; shall we, then, rejecting a *vera causa*, seek some other one, if indeed such other can be found? Cer-

3.



tainly not; and the conclusion is inevitable, namely, that a sheet of water must have stood at as many levels as there are buttresses, and this will include by short steps the whole space between the bottom of the valley and the lower shelf. Judging also from the amount of matter accumulated, we must infer that the water remained at these levels for no inconsiderable periods, although for a lesser time at each than at the level of the 972-feet shelf.

I would even further add, that in any valley (the relative level of the country, one part with another, having remained constant) a single buttress, if composed of such materials as could not have slided down the face of the hill in mass, or could not, judging by the presence of cross layers and alternations of fine and coarse beds, have been deposited by a debacle, indicates that the valley was once partly or entirely filled up to that height by such matter; and if the mass be too thick, or at too great an elevation on the sides of the valley, to allow of the supposition that it was deposited by the streams now flowing in the valley, subject to such changes in its velocity as by the corrosion of its own bed it could effect, then the formation of such buttresses can be accounted for only by the supposed permanence of a sheet of water, whether of a temporary lake or of the arm of the sea, at their levels. Now such projecting masses are extremely common in the sides of most of the tributary streams of the valleys. I conclude, therefore, from the consideration both of the beds of stratified alluvium at the bottom of the main valleys, which there is the greatest difficulty in believing could have been deposited by the rivers under the existing conditions; and of the buttresses on the sides of the hills, which similarly could not have been formed by the present streamlets, that it is satisfactorily proved that the valleys of the Spean and Roy have been occupied by a sheet of water which has slowly and very gradually retired, leaving in almost every part unequivocal evidence of the check which matter drifted by a current meets with, when it arrives at or near to the surface of still water.

I have as yet confined my argument to the valley of the Spean and its tributaries, and to that portion of it which is below the lower shelf; but I may here add, that it may be inferred from the same kind of evidence already used, (I allude more particularly to some buttresses above the 972-feet shelf to the north-east of the houses of Glen Turet, and to a shelf intermediate between the two upper ones, Tombhran,) that water long remained in Glen Roy at an altitude above that which we have as yet been considering, and at other levels besides those indicated by the three shelves themselves. If, also, we look to other valleys in this part of the country, we find similar appearances. For instance, on the flanks of the valley of the Tarf Water, which flows into Loch Ness (at the elevation of about 1000 feet, near the bridge, where the road to Garviemore crosses the river), there are large conical piles, with their summits truncated by a rude terrace, composed of well-rounded pebbles, sand, and an argillaceous earth in irregular beds. Some of the layers of sand and fine gravel were in curves, but slightly inclined; and this structure, together with their

composition, made it at once evident that they must have been drifted into their present position by currents of water. Again, near Fort Augustus, the Great Glen, with the exception of the central part, where the river has worn for itself a broad course, is filled with irregular strata about thirty feet in thickness of sand, gravel, and coarse shingle. In the sand some of the layers are most regularly waved, as if by a tide ripple. These beds are about seventy feet above the sea; fringes of similar deposits skirt at intervals both sides of the Great Glen, but where they are present they do not occur, as far as I was enabled to observe, at a greater height than about 100 feet, that is, than the water-shed of this great valley,—a fact somewhat analogous to the coincidence in level between the true shelves or roads and the heads of the valleys in which they occur. At the south-west end of the Great Glen, nearly opposite to Loch Leven, there are some extensive flats, which from a distance appear to be similarly composed, and which in one part have been modeled into two nearly regular terraces, one rising above the other. A somewhat similar structure may be observed in a part between Loch Eil and Loch Lochy; and this structure can only be explained by water having successively occupied for long periods different levels.

Referring now to more distant points, we find in the broad valley below Loch Tulla (a tributary of Loch Awe, and the stream flowing thence enters Loch Etive,) there are some appearances, although obscure, of the bottom of the valley having been once filled up with stratified alluvium. On the river Tay, however, near Loch Dochart, the phenomenon is clearly developed. On the south side there is a long mound or terrace, about 150 feet high, entirely composed of well-rounded pebbles mingled in layers with a yellow sandy clay. From this point to Tyndrum (at an elevation of between 400 and 500 feet above the sea) there are similar banks of waterworn materials, and in more than one part I observed a fine white sand, like that on the seashore. On each side of the valley where it divides, near Tyndrum, a broad expanse is scattered over with low ridges and flat-topped hills of equal height, from which it would appear that the whole space had once been covered up with these deposits. Towards the mouth of the Tay the terraces and platforms of Strathmore have been remarked by many observers, on the sides of the small neighbouring valley of the Dighty. Mr. Blackadder, in a letter to Mr. Lyell, says, "A narrow track of gravel, sometimes in the shape of platforms, at others in small hillocks, very similar in appearance to those of Strathmore, extends to the height of about 600 feet; and some isolated patches on the southern face of the Sidlaw Hills occur at a greater elevation." From expressions used by Macculloch and other writers, I am led to believe that beds of similar matter irregularly superimposed over each other, occur on the sides of almost all the valleys of Scotland. In such cases as in that of Loch Dochart, we have no proofs, as horizontal shelves or ancient beaches have not been preserved, that the relative level of the country has remained the same, since the period when it was first traversed by running streams; and therefore it is not absolutely certain that the present rivers, with a very different inclination, might not have deposited the

rudely stratified beds in the lower part of their courses, and afterwards with an altered velocity have cut through them. But as we do know that no such change has affected a large neighbouring region, and as such movements could hardly thus have influenced the drainage of valleys directed towards different quarters, such doubts may be overruled. This being the case, the same argument as before used may be repeated, namely, that the waterworn materials appear to have been transported by the present rivers, and yet that they are so deposited as could not have happened without some intervening cause. The phenomenon demands an explanation; and the only obvious solution is that which from several and nearly independent considerations was proved to have been the case with the Spean, namely, that it had been occupied by an expanse of gradually subsiding water, either of a lake or of an arm of the sea. This conclusion, therefore, may be urged with only little less force regarding many, if not all, of the valleys in this part of Scotland.

It may be asked, of what nature was this sheet of water? If we suppose a barrier erected across the mouth of each valley, and a lake to be thus formed, which sunk from the gradual deepening of its mouth, all the appearances above described would be explained. It is a startling assumption to close up the mouth of even one valley by an enormous imaginary barrier; to do this with all would be monstrous. Of such barriers in the district we are considering I need not say there does not exist any trace, nor need I repeat what I have already said against so vain a supposition as that they could have been swept away by any great debacle from the sides of those hills, of which the whole alluvial covering has been preserved since the period when the upper shelves formed beaches, without even a remnant of them being left; and I may add, that it will hereafter be shown by the clearest proofs, that the ordinary alluvial action, and likewise that of running water, even under the most favourable circumstances of a waterfall, has been far less efficient than could have been anticipated.

But it may be asked, would not the hypothesis of a succession of lakes explain the appearance, the matter accumulated above each delta sloping upwards from one level to another. I can only answer this with respect to those valleys which I have myself seen: in the Spean, Roy, Tarf Water, and some others, it is easy, as before stated, to replace in imagination the solid rock; and although some small lakes* would be

* Sir LAUDER has represented three in his map (Edinburgh Royal Transactions) by the figures 5, 6, and 7. I cannot, however, by any means agree with him in the limits thus assigned to them. Is it meant to be asserted, that there is any barrier perfect, with the exception of such a gorge as the river is now cutting, at the lower end of number (7), on a level with the line at its upper extremity; or so nearly so as to allow of the upper part being considered as a supralittoral delta? Such did not by any means appear to me to be the case. Was not the barrier only supposed to have existed, as in the theory of the shelves? I must also observe that the fringe or deposit does not terminate a little way within the mouth of the Roy, as represented by the line marked (7). It appears to me unfortunate that Sir LAUDER marked the limits of these deposits, which are accumulated in a *gentle slope*, in a similar manner as he has done the shelves, which are *horizontal*. Any one would suppose the lines 5, 6, and 7 were horizontal, like those marked 1, 2, 3, and 4. This difference alone indicates a corresponding one in their origin, as will hereafter be attempted to be shown.

thus formed by the replaced barriers (as probably would be the case in every valley), the fringe of stratified alluvium we are now speaking of skirts the valley at an elevation above them. To assume that these rocky barriers were formerly much higher, and were demolished by some means independent of the action of the river (for this action tends only to form a narrow wall-sided gorge, as may be seen in those barriers which certainly did exist), would be as gratuitous as the imaginary erection of one great barrier across the mouth of the valley, and would explain, from the continuity of the slope, the appearances far less perfectly. Moreover, if the origin of the sloping fringes could be explained by the assumed former existence of a chain of lakes, the buttresses high up on the sides of the valleys clearly could not be so. Nor will any one pretend that any lake-theory can be applicable to the deposits on the sides of the great valleys, such as Strathmore, and the Great Glen of Scotland, which terminate in deep and open friths. Therefore it has not been the water of several lakes any more than of one lake, which slowly retiring from these valleys, determined the accumulation of the beds, where we now see them. There is, then, as we have conclusive evidence that an expanse of slowly subsiding water did occupy these spaces, but one alternative, which we are compelled to admit, and this without any consideration of the *shelves* themselves, excepting so far as they serve as artificial levels to show that the country has not been unequally elevated, namely, that the waters of the sea, in the form of narrow arms or lochs, such as those now deeply penetrating the western coast, once entered and gradually retired from these several valleys.

Section IV.—*Proofs from organic remains of a change of level between the land and the sea in Scotland. The effects of elevation traced in hypothesis.*

Another question immediately arises; did the waters of the sea slowly subside, or the land slowly rise, the effect in each case being similar? But first it will be proper to show, from the more ordinary kind of evidence, that there has been some change of level between land and water affecting Scotland within recent times, although not to the amount inferred from the arguments above advanced. Mr. Smith of Jordanhill, in an excellent paper*, has lately shown from the presence of elevated organic remains, that within a period geologically extremely recent, both the east and west coast of Scotland has been raised some hundred feet; namely, at Banff and near Glasgow† about 350 feet. Considering the facts given in this paper, it can scarcely be doubted, without making the most improbable assumptions, that the Great Glen of Scotland, of which the highest point is only ninety-three feet above the sea, was within this recent period an open strait; and, I may add, it must then have strikingly resembled the Beagle Channel in Tierra del Fuego, an arm of the sea narrower, longer, and straighter, which intersects the extreme southern part of South America. In ac-

* Edinburgh New Philosophical Journal, vol. xxv. p. 376.

† Edinburgh New Philosophical Journal, vol. xxv. p. 386 and 387. The elevated shells at Banff were observed by Mr. Prestwich, Proceedings of Geological Society, May, 1837.

cordance with this fact, I was informed by the person who now has the charge of the locks on the canal, that when they were cutting through the gravel at the head of Loch Ness many broken sea shells were found in the *lower* part, which appeared to him like those on the sea-coast. When exposed to the atmosphere they soon decayed. This point must be between forty and fifty feet above the level of the sea. There are remnants, as before stated, in this part of the Great Glen, as well as at the south-west extremity, of coarse sublittoral formations, which, I suppose scarcely any one would dispute, were accumulated before that small change of level took place, which is indicated by the elevated marine remains. That the movement must have been exceedingly slow, may be inferred from the existence of so many beaches, each requiring time for its formation, which rise one above another on both coasts of Scotland. Mr. MALCOLMSON* mentions no less than eleven in Elgin, from the lower one of which he procured twelve species of existing marine Testacea. On the opposite coast also, Mr. Smith has described† several ancient beaches between the present one, and the great terrace, between thirty and forty feet high, which "forms a marked feature in the scenery of the west of Scotland." It is also important to observe here, that the supposed greater movement deduced from the nature of the superficial deposits, is of precisely the same slow kind, and interrupted (as will presently be shown) by periods of rest, as this lesser movement, attested by the presence of sea shells and step-formed beaches. If, then, the Great Glen was for a long period occupied by an arm of the sea, which very slowly retired from it, deposits must have accumulated on its shores, and likewise for some little distance within the mouths of the valleys which entered it. If we suppose that the sea stood at the same level in the Great Glen as it lately did both on the east and west coast, then the salt water would have almost entered Glen Roy, and would have wholly covered that sloping fringe of gravel, which has been so often mentioned as skirting the course of the Spean. Whether this be granted or not, after what has been stated it can hardly be disputed, that within recent geological periods an arm of the sea entered at least the mouth of the Spean, and very slowly retreated from it. Remembering that the conclusion was forced on us by distinct lines of arguments, that a body of water must have slowly retired from these valleys, and that lakes sufficiently large to have produced the observed effects could not have existed in them, may we not, with the additional consideration that some parts of the deposits here *must* be of marine origin, deliberately affirm it proved, that it was the waters of the sea that, even at great heights, checked and banked up at successive levels, the detritus brought down by the ancient rivers and streamlets? I am aware that the argument would have had a greater *appearance* of strength had I commenced with the inference deduced from the presence of recent shells at con-

* Proceedings of the Geological Society, 1838, p. 669. I was informed by an intelligent quarryman that he had observed many broken sea shells in a gravel-pit, about two miles north of Grant Town, on the roadside to Forres, and therefore eighteen miles from the nearest sea-coast.

† Edinburgh Philosophical Journal, p. 388.

siderable elevations on both coasts of this kingdom, but I preferred the method I have followed, because I believe it is equally legitimate, and of more general application, although at first not so obvious.

From these facts it is certain that there has been a change of level affecting within recent times the whole central part of Scotland, and of a kind very similar to that which has been the subject of so much attention in Sweden, where, according to Mr. Lyell, remains of existing marine animals have been raised to the height of between 500 and 600 feet above the sea. The change of level in the case of Sweden is as certainly known to be due to a slow movement of the land, and not of the water, as it is on the coast of Chile, where a small tract is violently upraised during an earthquake, the distant parts of the same coast being unmoved. It would, however, be quite superfluous here to enter into this question at length, as it has almost ceased to be debateable ground*. It may then be concluded that the supposed great change of level in Scotland, deduced from the foregoing arguments, as well as that smaller fraction of it attested by marine remains and ancient sea-beaches, is due to the rising of the land, and not to the sinking of the waters.

We will now endeavour to trace in hypothesis the effects which would be produced by an arm of the sea slowly retiring from inlets during an *equably progressive* elevation of the land. In a deserted sound or flat-bottomed valley, surrounded by mountains, curved lines crossing the river would mark the ancient beaches. Each of these lines would be higher than its neighbour on the sea-side, owing to the rising of the land in the interval of their formation, and would be more distant from the head of the valley, chiefly on account of the matter brought down by the river, and in some parts from the natural slope of the fundamental rock. When the upper line formed a beach, it is evident that the whole of the lower part of the valley must have been under water, and that the prolongation of the beach would stretch along the flanks of the adjoining mountains some way inland from the present shore. In like manner each successive and lower beach-line would wind along the steep sides of the hills, and cross the valley further and further from its head. It should be observed, that although I have spoken of successive beach-lines, yet as the land is supposed by the hypothesis to rise at a perfectly equal rate, every part of the valley will have successively formed, during an equal period, a beach; so that each part having been similarly exposed, the slope will be uniform; nor will it be possible to distinguish any one line of beach. Again, if we suppose matter to be removed from the valley by the action of the tides, instead of being added to it by the river, yet as an equal quantity (or a quantity insensibly varying from the varying degree of exposure, as the form of the land slowly changes during its rise) would be removed at each level, the slope in this case also would be uniform. In that part of each successive beach, which winds along the steep flanks of the mountains, it is not probable that much matter would be added, but the downward descent of some portion of the detritus, which is

* An excellent summary of the argument is given by Mr. Lyell in his Elements of Geology, chap. v.

formed on all land by meteoric agency, would be checked; but as it would be equally checked at each successive level, the outline of the mountain would remain unbroken. These same lines, however, although protected in the more inland parts, might suffer degradation where exposed to the greater force of the waves near the mouth of the sound; but the parts differently affected would blend into each other, and so would it be with each successive beach-line; and the slope therefore, whether added to or corroded, or left untouched, would never show the traces of action on any one defined horizontal line. A little reflection will indeed show that when the water stood at the highest level, any part or point which happened to be most exposed would, from the natural slope of all mountains, be some way inland compared with the same relative point on the present coast; at all intermediate levels the waves would attack an intermediate part, either high up and more inland, or lower down and nearer the coast, so that the line (or rather zone) of greatest littoral action, joining the parts which were successively most affected, would, under the conditions of the hypothesis, be inclined with the horizon either more or less, according to the original inclination of the land. Lastly, the river in the valley, as it gained power from the sinking of the sea, would generally remove the central portions, and leave only a fringe of the littoral and sublittoral deposits. This fringe, although formed by successive horizontal beach-lines, would *slope* upwards, as the whole bottom of the valley would have done if no part had been removed. I allude to this structure more particularly, because it is not at first obvious that matter accumulated on a sea-shore would in any case form a fringe of this kind.

In the hypothesis I have supposed the upward movement of the earth to have been absolutely uniform during equal periods. But this probably has seldom been the course of nature. There is clear evidence that the action of volcanos is intermittent; and the force which keeps volcanos in action being absolutely the same with that which elevates continents (as I endeavoured to prove in a paper read not long since, March 7th, 1838, before the Geological Society), so we must suppose that the elevation of continents is likewise intermittent,—a conclusion which receives ample confirmation from the occurrence in nature of successive lines of escarpment, rising one above another, which mark those periods of rest when the sea wore deeply into the former coast. Let us then suppose that the water stood for a longer time at some one level than at any other. The first effect would be, that the beach or delta at the head of the sound, where the river is constantly bringing down detritus, would be broader there, owing to the greater accumulation of matter during this longer period, than in any other part; and therefore when the bottom of the whole valley was converted into land, the slope, which is everywhere gentle, would in that part approach nearer to horizontality; but in other respects there would be scarcely any difference. In like manner, in those portions of the mountains, on each side of the valley, where from the protected nature of the site matter did during the whole rise accumulate, though very slowly, the line would, from the greater quantity of matter added during

the longer period of rest, slightly project beyond the general slope of the surface; and where any rivulet came down a very little delta would be formed. Also on any projecting or exposed point, the solid rock would be more deeply cut into than in the other lines. But as the land rose, the little deltas gently sloping from the line of ancient beach, with their front part cut off by the action of the subsiding waters, would project from the hill sides in the form of obliquely truncated buttresses; to the heads of which the horizontal lines of beach will exactly coincide, as indeed they likewise will with the broader ones, where crossing the bottom of the main valleys; but the slope in the latter case will blend both above and below with the inclined surface formed by the matter rapidly accumulated at every successive level. Now it has been shown that Scotland within modern times has undergone a great elevation; it has been shown to be extremely improbable that such movements should be equally progressive: the effects of aqueous action on the surface of the land during the intermittent periods of rest in the elevatory forces have been traced; and it will have been perceived by those who have read the early part of this paper, or the memoirs of Sir Lauder Dick and Dr. Macculloch, that the results anticipated in the hypothesis are the characteristic features, even in detail, of the "parallel roads of Lochaber": I believe, then, that the hypothetical case gives the true theory of their origin.

Section V.—*Objections to the theory from the non-extension of the shelves, and the absence of organic remains at great heights, answered.*

Several objections to this view, which implies that the whole country has been slowly elevated, the movements having been interrupted by as many periods of rest as there are shelves, will occur to every one. Perhaps the most important of these is, that, as the upward movement probably affected a considerable area, or at least as it cannot be supposed to have been confined within a defined line, so ought the shelves to be continuous over an equal space. I believe, however, from what I have seen in South America, that it would be more proper to consider the preservation of these ancient beaches as the anomaly, and their obliteration from meteoric agency the ordinary course of nature. Some contingencies seem absolutely necessary for the formation of the shelves, such as a sufficient height in the land, a steep slope, and that the country should be formed of rocks which afforded an abundance of somewhat adhesive detritus; we may conclude, moreover, that the surface must have been covered with turf, *immediately* after the waters subsided; for otherwise the loose matter would infallibly have been washed from the hills, and this contingency implies a protected, and hence, perhaps, an *inland* situation, which, at the period, when the water stood at the upper shelves, would leave but a small area. The abundance of detritus no doubt is quite necessary; for although the solid rock is in some parts notched, I do not believe the shelf would anywhere be distinguishable if the soil and detritus were entirely removed from it. It would also appear to be necessary that the valley should either have been originally closed at its upper end,

or that during the period of rest some shallow part in it should have become so from the accumulation of sediment, or from any other cause, so that no stream set through it. Thus the two upper shelves of Glen Roy die away as soon as they enter the valley of the Spean, which must at the period when the waters stood at their levels, have formed an open channel connecting opposite seas. That the ancient beaches in this case extended to that point, beyond which the accumulation of matter was prevented by too much exposure, seems clearly indicated, in a manner before explained, by the extremities of the lower shelf stretching beyond those of the upper. When, however, the 972-feet shelf existed as a beach, the channel of the Spean was converted by the closing of the pass of Muckul into a sound; and the shelf, apparently in consequence, winds along the sides of the valley both of the Spean and Roy. Besides the requisites here mentioned, the shelves appear to be more plainly marked where the valley is narrow, and, perhaps, likewise where it is tortuous. Now from the little I have seen of Scotland, I very much doubt whether these several contingencies occur frequently together; they certainly did not in several valleys which I visited. It must also be borne in mind, that as Sir Lauder Dick traced the lower shelf very much further than Macculloch had done, and as I found a remnant of one in a distinct valley, and especially as Sir David Brewster has seen shelves in two places on the Spey, the probability is that others, though perhaps obscurely developed, will yet be discovered. The irregularly shaped area, in which shelves have already been found, measures in one line twenty British miles, and in another twenty-five.

Notwithstanding what I have now said, the presence of the shelves in some of the glens and their absence in others, in the district of Lochaber itself, is a very extraordinary circumstance. Thus in Glen Roy three lines are perfectly developed, whilst in the neighbouring one of Glen Gluoy it appears that only one exists. It is useless without data to speculate on the nature and force of the tides, currents, and winds of former periods, or on the kind of vegetation with which the land was then covered; all circumstances, perhaps, sufficient to determine the formation or preservation of a mere narrow mound of soft matter on the steep side of a mountain. But the following case proves, and it deserves particular attention, that the limits of the ancient waters cannot even approximately be inferred from the present extension of the ancient beach-lines. Macculloch has drawn in his map a shelf intermediate between the two upper ones, on the face of the mountain (Tombhran) opposite to where Glen Turet joins Glen Roy: Sir Lauder Dick has not noticed this shelf*. Perceiving its

* Until I saw this shelf I doubted its existence, because I had not been able to discover others mentioned by Macculloch: thus one is figured by him in a ravine branching from Glen Roy (improperly called by him Glen Fintec), which, though having ascended it, I was unable to see. Again Macculloch states, that two shelves occur in Glen Gluoy, whilst Sir Lauder Dick, who seems to have examined most carefully this glen, could find only one. I may here remark, that should two shelves be hereafter discovered there at the same relative height from each other with those of Glen Roy, and this is stated to be the case by Macculloch, the fact would be highly satisfactory on the theory of the shelves having been sea-beaches. From an excellent point of view, however, on the side of Ben Erin I could see no trace of a second shelf. Macculloch also

importance I examined it with scrupulous care. It occurs rather nearer the lower than upper shelf, and as these two are only eighty-two feet apart, and are here strongly marked, it was scarcely possible (especially as I purposely looked at it from *every* point of view,) to make any mistake in the absolute parallelism of this intermediate shelf. It can be traced for nearly three quarters of a mile; at the west end disappearing quite insensibly, like the lines in Glen Collarig, but at the other end rather more abruptly in a water course. I walked along its whole length, and its structure is perfectly characteristic; I refer to the materials of which it is composed, its breadth and inclination. The two regular shelves are, perhaps, more plainly marked here than in any other part of the whole glen; and it would appear probable that this is owing to that portion having been exposed to a longer space of open water, by which means the ancient waves acquired a greater than ordinary power in heaping up detritus. In the mouth, however, of Glen Collarig and of Glen Roy, an exposure to a wider channel, but at the same time to one open at both ends, and therefore probably a tide-way, has entirely prevented the accumulation of matter; and hence the beaches gradually disappear there. This view, if correct, as I fully believe it to be, shows by what a slight difference of circumstances, either a remarkable development or an entire obliteration of the ancient beaches has been determined. The intermediate shelf clearly owes its existence to the same causes which have in this part so strongly marked the upper and lower one; and though it is less strongly marked than these two in this immediate neighbourhood, yet it differs but little from them as they ordinarily occur, and is, I think, fully as plain as the lower shelf throughout Glen Spean. I assert, then, that it is an incontestable fact, that water must have remained at the level of this intermediate shelf for a long period, and only a little less long than at the other lines; yet in no other part of Glen Roy, the valley where circumstances have been so pre-eminently favourable for the formation and preservation of these beaches, a trace of this intermediate shelf has been observed. It has likewise been most clearly shown, that barriers could not have existed at the double mouth of Glen Roy, and we have seen that the surface of the land has been preserved in that neighbourhood in a manner quite extraordinary; yet it is known on the authority of Sir Lauder Dick, who appears to have examined the whole course of the Spean and its tributaries with great care, that not a vestige of either of these upper shelves can be discovered beyond the mouths of Glen Roy. Any argument, therefore, whatever, from the non-existence of the shelves or beaches bearing on the former limits of the ocean over this part of Scotland, during the period of rest in the subterranean movements, is valueless.

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figures a supernumerary shelf at a point north-west of the houses of Glen Turet, at a level above that of the upper shelf of Glen Roy; a mound of alluvium, above, and nearly parallel to the shelf, certainly occurs there; but from the want of sharpness of outline, I should be unwilling to pronounce that it had formed a line of beach, although I should be far from feeling any surprise if this could be shown to have been the case.

In the valleys of the Spean and the Roy, I attentively examined, with the expectation of finding fragments of sea shells, the matter accumulated on the shelves, and more especially the thicker beds of gravel and sand which occur at lower levels; but I could not discover a particle, and the quarrymen assured me they had never observed any. This may at first be thought a strong objection against the theory of the marine origin of these deposits. But having been led in consequence of Mr. MURCHISON's remarkable discovery of recent sea shells in the inland counties of Shropshire and Staffordshire, to examine many gravel pits there, and having observed how frequently it happens, that not the smallest particle can be discovered in vast accumulations of the rudely stratified matter, and that when found, the fragments are generally exceedingly few in number and partially decayed, I feel convinced that their preservation may be considered as a remarkable and not as an ordinary circumstance. After a longer interval of time, or under some slightly less favourable conditions, all the gravel beds of Shropshire, which no one can doubt were accumulated beneath the sea, would be as destitute of organic remains as those of Lochaber. In some parts of South America I have found beds of gravel which did not contain a fragment of shell, and yet on the bare surface, nearly perfect ones were strewed in numbers. Mr. SMITH describes * beds on the west coast of Scotland, and Mr. LYELL † others in Sweden, undoubtedly of marine origin, but wholly destitute of organic remains. On the coast of Forfarshire also Mr. LYELL, as I am informed by him, found shells in gravel beds extending to the height of between fifty and sixty feet; but at greater altitudes similar beds occur which do not contain any: he has observed the same kind of fact strikingly illustrated in Norway ‡. It is easy to imagine several

* Edinburgh New Philosophical Journal, vol. xxv. p. 380.

† Transactions of the Royal Society, 1836, p. 11. and 15.

‡ Mr. LYELL has had the kindness to give me the following observations on this point.

"In the country surrounding the fiord of Christiania, especially between Christiania and Dramman, and between Dramman and Holmstrand in Norway, deposits of clay and sand rest in horizontal beds on the gneiss, granite, porphyry, and other rocks. Large masses of this sand and clay reach in some places to elevations of more than 600 feet above the level of the sea, and nearly fill many upland valleys; but it is only in those patches which occur at the height of about 200 feet, and usually less than fifty feet above the sea, that shells (all of recent species) have been found. This sand and clay appear to have accumulated on the older rocks during their gradual upheaval from beneath the sea, so that greater elevation becomes a test of higher antiquity, and those patches which are found at small heights near the borders of the present fiord are very modern. Even in these last the shells are often in so advanced a state of decomposition as greatly to favour the theory that a more considerable lapse of time might be sufficient to obliterate all traces of their existence. Thus for example, on the banks of a small river about two miles above Töusberg at the place where the bridge crosses it, a section of loamy clay is laid open, the lowest part of which cannot be raised more than a few feet above the salt water of the fiord of Christiania. In the upper part of the mass for a thickness of fifteen feet no fossils can be detected, but somewhat lower faint casts of the *Mytilus edulis*, chiefly indicated by purple stains, are observable. Still lower down more perfect specimens of the same shell, together with *Cardium edule*, occur, but both in so soft a state as to crumble into dust when dried. With these the more solid *Cyprina islandica* and *Saxicava rugosa* are occasionally found, and although soft when first taken from the matrix are capable when dried of being preserved entire. If in the short period which has probably passed away since these shells

circumstances which might determine the preservation or decay of the shells; even on the assumption, which is not necessary, that they have in all such cases been imbedded. Thus in Shropshire, the gravel is covered in most parts by an earthy deposit, which contains a small proportion of lime; hence the rain water having absorbed carbonic acid gas in its descent, would find matter to dissolve before it reached the layers containing shells; whereas in Lochaber the gravel and sand, being derived entirely from granite rocks, does not, as I ascertained, usually contain any free carbonate of lime, and consequently the fragments of shells would more readily be dissolved. I do not wish to assign this circumstance* as the real cause of their disappearance, but merely to indicate it, and other similar ones, as quite sufficient to show that the marine origin of the shelves cannot be controverted from the absence of organic remains.

Section VI.—*Application of the theory to some less important points of structure in the district of Lochaber, and recapitulation.*

By considering the hypothetical case above given, I think it was shown that the proposed theory explains every essential point in the phenomenon of the parallel roads. And I will now endeavour to show how far it applies to some minor points of detail. For instance, I have described a horizontal band of rock on one side of the narrow mouth of Loch Treig, with its face worn into smooth concave forms, like those over which a water-fall rushes; and on the other side, a great spit or bank of sand and gravel. Now on the belief, that a sheet of water seven or eight miles long, and two or three broad, was drained during each ebb-tide to the depth of several feet through a narrow curved channel, and then again raised by the following tide to its former level, the effects there produced are quite intelligible. It is also easy to perceive, that through the means of the tidal action, points of solid rock might have been obliquely cut off in the same manner as on existing beaches; and that flat channels, resembling in every respect those which at present frequently separate small

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near Tōusberg were imbedded, the progress of decay can have proceeded so far, we may well suppose the percolation of water during antecedent ages of indefinite extent to have destroyed all signs of fossils in the more ancient and elevated patches of loam found more than 500 feet high in the adjacent hilly country."

* I may observe that it very frequently happens, that shells are found only at some depths in these superficial deposits: this is the case in several of the gravel pits in Shropshire; in cutting the canal at the head of Loch Ness, the shells were met with at the bottom, whereas, the layers nearer the surface, as I can vouch, contain none. Mr. Smith speaking (p. 380 and 391. vol. xxv. Philosophical Journal,) of the clay beds on the west coast of Scotland, says, that the marine remains with which it abounds "are almost invariably found in the lower part of the bed." I infer that in all these cases shells originally existed in the upper part, but have since decayed: Mr. Smith, however, offers a different explanation. In the extensive and superficial beds of elevated shells on the coast of Peru, where rain does not fall, and where consequently loose matter is not washed from the surface, I have traced as I have ascended from the beach a most perfect gradation in the decay of the shells, until a mere layer of calcareous powder, without a vestige of structure, alone remained.

islands from larger ones, might have been worn between hummocks (such as those on one side of Meal-derry) and the lines of shelf.

If, again, we consider what must take place during the gradual rise of a group of islands, we shall have the currents endeavouring to cut down and deepen some shallow parts in the channels, as they are successively brought near the surface, but tending from the opposition of tides to choke up others with littoral deposits. During a long interval of rest in the upward movements, from the length of time allowed to the above processes, which essentially require time (though they are favoured by the *rise* of the land rather than by its remaining stationary), the tendency would often prove effective both in forming by accumulation of matter, isthmuses, and in keeping open channels. Hence such isthmuses and channels just kept open, would oftener be formed at the level, which the waters held during the interval of rest, than at any one other. These isthmuses and channels when left by the receding waves, might be called land-straits, for they would present smooth, flat, narrow surfaces, connecting more open spaces. During the rise of the land they would at first separate the heads of two adjoining creeks, and afterwards, the upward movements proceeding, they would form the watersheds between adjoining and opposite glens. By this means, I explain both the ordinary structure of the land in these mountains, where the waters divide, as already described; and more especially the remarkable fact of the exact coincidence of several such points with the lines of shelves,—the shelves only indicating the long interval of rest in the upward subterranean movements. It may be remembered that I described at the head of the Roy and of the glen near Kilfinnin, patches of alluvium or remnants of terraces on the sides of the land-straits, a little above the flat where the waters divide. This structure is in perfect accordance with the theory that drift matter began to accumulate in such parts at that period, when the tides in them were first checked, or otherwise affected by the rising of the land; and that the channels were finally closed at their present levels, solely from the long interval during which the sea acted at such levels. Hence, also, we might have expected, that patches of alluvium would occur (as is the case) on the sides both of the land-straits which are, and those which are not connected with shelves at corresponding levels.

From the levels taken by Mr. Maclean with Sir Lauder Dick, it appears that the upper limit of the Glen Gluoy shelf, which coincides with the division of the waters, is twelve feet higher than that of Glen Roy. The intervening space is nearly a mile in length, moderately broad, and very flat, having only a fall of the twelve feet; and Sir Lauder states* that he saw in this part the surface of the solid rock in the bed of the little stream. These facts seem at first to indicate that two periods of rest had supervened, one when the water stood at the level of the Glen Gluoy shelf, a second when at the upper level of Glen Roy after a rise of twelve feet, and that, nevertheless, the effects of these two periods of rest were confined respectively to separate, though closely adjoining glens. This circumstance if so interpreted, although improbable in

* Edinburgh Transactions, vol. ix. p. 35.

the highest degree, could not be considered as subversive of the theory, after it has been ascertained that the upper shelves of Glen Roy are not prolonged into the valley of the Spean, and that the short intermediate one in Glen Roy does not extend for more than three quarters of a mile in that valley. There is, however, I suspect a more satisfactory explanation. In the First Narrow of the Strait of Magellan, the tide rises about forty feet, as Captain FITZROY informs me, whilst eighteen miles to the west at Gregory Bay, the rise is only about twenty feet. Here then, and other instances might be adduced, in a distance of eighteen miles, the surface of the water must slope no less than twenty feet. Let us suppose a *rocky* barrier (and that of Glen Gluoy is rocky) to be elevated, by such movements as those now in progress in South America, across the strait, separating it into two portions. Might we not expect that the high water mark would rise several feet higher, in that portion of the former channel which was still open to the sea subject to the great tidal movement, than it would in the other connected only by tortuous passages with a different sea, where the rise of the tide was small? In such a labyrinth of channels as this part of Scotland must have presented when the sea stood at the level of the upper shelves, it is even probable that there would be inequalities in the rise of the tide in different parts; I conclude therefore that when the rocky barrier was upraised between Glen Gluoy and Glen Roy, a greater tide-wave, proceeding direct from the line of the Caledonian Canal, then a great strait, swept up this deep creek; whereas a smaller one reached by a circuitous course the *Bay* of Glen Roy, which, moreover, was connected by some other straits with the eastern sea.

Whoever walks over these mountains, and believes that each part has been successively occupied by the subsiding waters of the sea, will understand many trifling appearances, which otherwise, I believe, are unintelligible. Thus in Upper Glen Roy he will see in the level expanse, an old bay, filled up and leveled with tidal mud. Again at the Gap of Glen Collarig, with its flat bottom and cut off sides like a gateway, he will recognise a channel, at last choked up with matter drifted by the tides, and now left in the state in which it was when the waters retired from it. The traces of supernumerary shelves will offer no perplexity to him, and will equally receive with the others a simple explanation. By the theory of the sea having acted at successive levels over the whole surface of the land, the great beds of shingle* and sand,

* I have before alluded to the fewness of the well-rounded pebbles near the upper shelves, excepting at the heads of the valleys, or on flat places. This is a difficulty; though it is one common to many regions, where we know that much denudation has taken place at some period or at another. Pebbles of most rocks may in the course of time decay, but those of quartz I should think (although SCORESBY says this rock yields to the frosts of Spitzbergen) would be imperishable: if so, how comes it that quartz pebbles are not scattered over the surface of every mountain in which that rock is present, and in which the form of the land, its denuded state, or the presence of truncated dikes show that it must once, although perhaps countless ages since, have been beaten by the waves of the sea? Such pebbles, however, are not found on every mountain thus circumstanced: the explanation, I presume, rests in this; that every cause of disturbance, wind, rain, earthquakes and the fall of fragments all tend to move the pebbles in one direction alone, namely downwards. I am inclined to believe this view is

such as those near the mouth of the Spean, have a cause assigned to them adequate to the effect. Lastly, the manner in which the deposits near the mouths of the larger valleys have been modeled into successive terraces, which in some parts at least appear not to have been formed by the river, receives elucidation. I may add, that in South America I have observed numerous instances of terraces in every respect similar to these, with sea shells abundantly scattered on their surface; and therefore where there could exist no obscurity regarding their origin.

In concluding this part of my paper I will recapitulate the course of the argument pursued. 1st. It is admitted by every one that the horizontal shelves are ancient beaches. 2nd. I showed that no lake theory could be admitted on account of the overwhelming difficulties in imagining the construction and removal at *successive* periods of *several* barriers of immense size, whether placed at the mouths of the separate glens, or at more distant points. 3rd. The alternative that the beaches, if not formed by lakes, must of necessity have been so by channels of the sea, was not advanced, only because it was thought more satisfactory to prove from independent phenomena, that a sheet of water *gradually subsiding* from the height of the upper shelves to the present level of the sea, occupied for long periods not only the glens of Lochaber, but the greater number, if not all the valleys of this part of Scotland; and that this water must have been the water of the sea. 4th. It was stated (the strongest argument being the ascertained fact of the land rising at the same time in one part and sinking in another,) that in all cases the land is the chief fluctuating element; and, therefore, that the above change of level in Scotland, independently attested by marine remains at considerable heights on both the eastern and western coasts, implies the elevation of the land, and not the subsidence of the surrounding waters. 5th. It was shown that in all such prolonged upward movements it might be predicted, that there would be intervals of rest in the action of the subterranean impulses. 6th. By an hypothetical case, the land was subjected to the above conditions, and its surface was found to be modeled in a manner wholly similar, even in detail, to the structure of the valleys of Lochaber as they now exist. 7th. The true theory being considered thus established, objections to it from the non-extension of the shelves, and from the absence of organic remains at great altitudes, were answered and shown not to be valid. 8th. Many points of detail in the structure of the glens of Lochaber, were shown to be easily explicable on the supposition, that the valleys had been occupied by arms of a sea subject to tides, and which had gradually subsided during the rising of the land. Having attentively considered these several and

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correct, and that in the course of time, such pebbles are all rolled down, from having found on an isolated mountain of quartz in South America (the Sierra Ventana) a superficial patch of conglomerate, like part of an old beach, which seemed solely to owe its preservation to the pebbles having been cemented to the parent rock by oxide of iron, in the same manner as not unfrequently may be observed on some existing sea beaches. In the case of the shelves of Lochaber, it is probable, that only a few pebbles were originally formed, owing to the small power of the waves on the steep and protected shores of these ancient sounds.

independent steps of the argument, the theory of the marine origin of the "parallel roads of Lochaber" appears to me demonstrated.

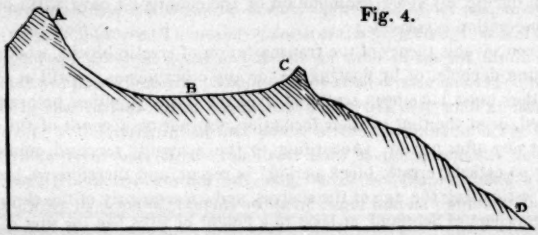
I may here remark, that MACCULLOCH seems to have been aware of the great difficulties attending his theory: but having proved that the roads could not be works of art, or the effects of any great debacle, he argued, to use his expression from the dilemma of the case, that they must have been formed on the shores of a lake. The idea of a continent slowly emerging from beneath the sea, appears, and it is a very curious point in the history of geology, never to have occurred to him as a possibility, although he was so bold and ingenious a speculator. His paper was read in the beginning of 1817, and when we reflect that during the few latter years, proofs of such movements have accumulated from all quarters of the world, we must recognise how much of this all important change (the foundation-stone, I may add, of this paper) is due to the Principles of Geology by Mr. LYELL.

Section VII.—*On the erratic boulders of Lochaber.*

I will now pass on to some other considerations which partly derive their interest as dependent on the truth of the foregoing theory. I have said, that the parent rock of many of the fragments lying on the shelves is not found in the immediate neighbourhood. These erratic boulders are generally of granite, and are from one to five and six feet in diameter; they are not confined to the shelves, but are scattered on the sides of the mountains. On the summit of the insulated hill of Meal-derry, above the level of the 972-feet shelf, there was one of large size, together with some well-rounded pebbles of rocks, which, I believe, do not occur there. In the gap of Glen Collarig the boulders on and near the upper shelves are frequent, as they likewise are in the pass between Upper and Lower Glen Roy; they occur also abundantly at the bottom of the latter valley, and on the side of Tombhran. From having found them in almost every part which I examined, I have little doubt that they are distributed in numbers over all the valleys and mountains, at least, to an elevation as great as that of the upper shelves: I make this latter restriction, because having ascended the mountains only in a few places above that level, I cannot speak positively with respect to the greater heights. On the mountains, however, between Glen Roy and Glen Gluoy on a hillock north-north-west (*magnetic*) of the summit of Ben Erin, I found several masses of granite, one of which was four feet by three in width and two in thickness (together with a couple of pebbles from rocks not *in situ*) resting on the surface of the gneiss. This hillock seemed to be entirely composed of the latter rock, and it was separated from all other hills by a valley. On the flanks of Ben Erin at about the same level, there were several boulders of granite, one of which was six feet across. Of those on the hillock (probably there were many others which I did not see in merely crossing the mountain,) the highest one was found by comparison with the Glen Gluoy shelf (by means of the barometer), to be 2200 feet above the level of the sea. I will describe in detail the spot where I found one other boulder,

in as much as the whole of the district being composed of gneiss, it might be suspected that patches of granite occurred high up on the slopes of the mountains, and that the fragments had simply rolled down into their present positions. This, however, could not have happened in the case last described, nor in the following one: about twenty feet below the summit of a very sharp peak (1600 to 1700 above the sea) the whole of which consisted of tortuous layers of gneiss, there was a block of syenite with pink felspar, two feet eight inches across. The peak is wholly separated (as shown in the wood-cut, fig. 4.) from a lofty mountain also of gneiss, by a *broad* and quite flat valley, the highest part of which is 215 below the spot where the boulder lay. I may observe that I did not anywhere see another boulder of the syenite, nor a single one of granite on this side of the mountains, which is separated by a lofty ridge from the valleys of Glen Roy and Glen Gluoy, where the blocks of granite are so numerous. Between two branches, however, of the Tarf Water (which enters Loch Ness near Fort Augustus) on the summit of a hillock of gneiss, about 1200 feet above the sea, I noticed one of granite.

Fig. 4.



A. Lofty mountain of gneiss.
B. A peat moss 215 feet below the boulder, dividing the waters flowing on each side round the hill C.
C. Boulder of syenite resting on gneiss 1600 or 1700 above the sea.
D. Habercalder in the great glen of Scotland.

The granite of all the boulders which I observed in Glen Roy, and likewise of those on Ben Erin, has a uniform character; it is subject to much disintegration, and therefore I do not doubt that the boulders were originally much larger. In MACCULLOCH's Geological Map of Scotland, the nearest granite *in situ* to the boulders on Ben Erin is seen to be at the source of the Roy, near Loch Spey, a distance in a north-east line, passing over mountain and valley, of between five and six miles. The granite there has the same lithological character with that of the boulders, and I do not doubt that it is the parent rock, at least, of those strewed along the course of the Roy. With respect to the boulders on Ben Erin, they are completely cut off from every granitic district by valleys, the *highest* point of which is 920 feet below that boulder, the altitude of which I measured; that is, it would be impossible to walk from granite *in situ* to these boulders without ascending at least that number of feet.

I will only further add, that if a sheet of water were raised to the level of the Ben Erin boulders, there would be a line of open communication* between them and the granite of Loch Spey; although I must confess I much doubt whether in that case any of the rock *in situ* at Loch Spey would remain uncovered; and if so the origin of the boulders must be more remote. The other tracts, where granite is represented in MACCULLOCH's map, are more distant, and are separated by deeper and broader valleys from the points in question. From my limited examination of the district of Lochaber I am unwilling to generalize respecting the position of the boulders, but I think that they certainly occur most frequently on the summits of little peaks, such as on Meal-derry, or on that one of which a wood-cut has been given; and perhaps likewise in the narrowest parts of the valleys; for instance at the junction between Upper and Lower Glen Roy. I observed also a greater number on the shelves than I should have anticipated, from some of those, which had originally stood higher, having rolled down. But, I repeat, I will not positively say that such is the case; although with respect to the boulders on the peaks, as I observed five well-marked cases, even during my short examination of the country, I have little or no doubt that the observation is correct.

On any conceivable theory of the transportation of erratic blocks, whether by some overwhelming debacle, or by floating ice, or any other means, it will at once be evident that they must have been scattered over the country, either before the shelves were formed, or at the time of their formation, but not on account of the delicacy of the lines at any after period. According to the generally received opinion of geologists, the so-called "erratic block period" is recent, and therefore we obtain a rude method of estimating the age of the shelves, and consequently of the elevation of the whole central part of Scotland, at least to a height of 1278 feet (or that of the upper shelf) above the sea.

It may perhaps be worth while briefly to compare together, under the conditions here afforded, the two theories of the transportation of erratic boulders, which are alone worthy of consideration, namely, that of great debacles and of floating ice. I will not lay any stress on the difficulty of imagining, in accordance with the first theory, a rush of water so impetuous as to transport vast masses of rock across profound valleys and up the steep sides of high mountains, for this difficulty has no special reference to the case of Lochaber; but those who believe in the past occurrence of so terrific an agitation of the waters of a deep sea, must in some manner account for the frequency of boulders in the most exposed places on the summits of hillocks, and likewise for so many having been left in narrow straits, where one would have anticipated the most impetuous rush of water. On the face of Tombhran I observed many boulders scattered on the shelves, which have been formed there not

* This is a similar fact to what has been observed on the Jura. Sir JAMES HALL (Edinburgh Royal Transactions, vol. vii. p. 143.) says "it is principally where the snowy summits are visible from the face of the Jura by means of some depression in the intervening hills, that we find these travelled masses."

only by the accumulation of loose matter, but also by the deep excision of the solid underlying rock. Again, there were other boulders on the shelves on the rocky peninsula near the junction of Upper and Lower Glen Roy, where much of the gneiss has been worn away. Here it was not possible, from the non-existence of higher land, that the boulders could have rolled into their present places from above, after the formation of the shelves; nor was this at all probable in several parts of Tombhran. On the supposition of the boulders having been originally scattered over the country, and the shelves formed at a subsequent period, we have the difficulty, though perhaps not an insuperable one, as we do not know their original size, of believing that blocks of granite have been preserved for a long period on those very places, where a zone of gneiss had been cut into and worn away. Some of the boulders on Tombhran were lying on the surface of the lower edge of the shelves, in parts where, as above said, I fully believe the inclination of the ground was so trifling that it was impossible they could have rolled down from above; but I regret much that I omitted, from not having perceived its importance, to ascertain this point with certainty. If the fact be so, and I scarcely doubt it, it would prove that some action, so quiet as not to have disturbed the small quantity of earth and little stones, of which the shelves are formed, transported these boulders across deep arms of the sea, and left them on the surface of the ancient beaches. The theory, that all erratic blocks, circumstanced like these of Lochaber, have been transported by floating ice, wholly removes these difficulties; for the icebergs, in the first place, would generally land the fragments, with which they were charged, on the lower part of the beaches or shelves; and secondly, those which had arrived not long before a fresh elevation would have been exposed only to a small amount of tidal degradation. Thirdly, the icebergs would frequently be stranded on shoals and islets, over and round which the tides swept; and likewise they would be frequently driven on shore in the narrow parts of the channels, where the waters were pent up. So that in after times, when the land was drained, it is easy to perceive that the boulders would lie scattered in such places, as they now actually occupy in the district of Lochaber. Lastly, this theory requires that every district where boulders are found should have been covered by the sea; here we have independent proofs that such was the case, at least to an elevation of 1278 feet.

In my Journal during the voyage of the Beagle, I have endeavoured to show that the erratic blocks of central Europe were probably transported at that period*, when

* I refer, of course, only to the more temperate and central parts of Europe, but it appears that boulders are sometimes transported in these regions, even at the present time. Sir James Hall, in his Memoir on the "Revolutions which have affected the surface of the earth" (Edinburgh Transactions of the Royal Society, vol. vii. p. 157.), states that in the Solway Firth (and therefore in salt water) "a large block of stone, four or five feet in diameter, lying within high-water mark, and well known as having served as the boundary of two estates, was during a stormy night in winter transported ninety yards, and the persons on the spot were convinced that this migration was performed by means of a large cake of ice, formed round the stone, and attached to it, and that the whole had been lifted and carried forward by the rising tide. The course of this stone was

its climate was more equable (chiefly consequent on the larger area of water), which favours a low limit of the snow line, and therefore the probability of glaciers, the parents of icebergs, descending in favourable places into the sea. It is therefore to this period, if this view be correct, that we must refer the "parallel roads of Lochaber," and consequently the elevation of the land, not only of the 1278-feet portion (which it is certain has been elevated at an epoch not distant), but likewise of the whole altitude, whatever it may be, at which boulders occur. If there be others, as is most probable, at a greater height than that one on Ben Erin, which I observed in merely crossing the mountains at a point 2200 feet above the sea, then by so much the greater has the elevation of the land been within this same period. Mr. BLACKADDER (in a letter to Mr. LYELL) states he has seen on the west coast of Scotland, in the island of Mull, large fragments of quartz rock at the height of 2000 feet, of the same description as that found on some of the adjoining islands and mainland. In Sweden M. SEFSTRÖM says that boulders occur at an elevation of 1500 feet; in Massachussets, in North America, they are found, according to Professor HITCHCOCK, at 3000; and on the Jura it is well known they occur, from low down, to an altitude of 4000 feet. It is interesting to discover, that in our own country the upward movements, within *the same period*, have been more than half as great as those which have affected the latter colossal chain. But regarding the exact period, allowance must be made, since on the one hand the glaciers of the Alps, situated ten degrees nearer the equator than those on the mountains of Lochaber, must have much earlier retreated upwards, and failed in descending to the level of the sea, during the change from the former to the present climate; whilst, on the other hand, to counteract the equatorial influence, they were appendages on a greater mass of snow accumulated on far loftier chains.

Section VIII.—*On the small amount of alluvial action since the formation of the shelves.*

I now pass on to another consideration. MACCULLOCH was much struck with the fact, that in many cases where a shelf crossed a rivulet, I mean one of those silver-like threads of water which descend the flanks of steep mountains in nearly straight

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marked upon the sand below by a deep and broad furrow, which remained visible for a long time afterwards, as I have been informed by several members of the Society, who saw it after an interval of more than a year." I presume from the position of the stone as a land-mark, and from the distance it was transported by the *rising* tide, that the furrow left by its passage must have been either oblique or parallel to the shore. What would have been the effect if this large and heavy block had been pushed over a surface of solid rock instead of sand? This question will recall to the mind of those who have read the late papers of Messrs. CHARPENTIER, VENETZ, and AGASSIZ, the case of the longitudinally and obliquely scratched rocks of the Alps. In the Addenda to my Journal during the voyage of the Beagle, I have endeavoured to show that the passage of ice, with imbedded fragments of rock, acting at successive levels on the surface of shoals during the gradual rising of the land, offers the most probable explanation of the scratches and grooves, which have justly excited so much attention in Scotland and other places.

lines, it frequently entered a little way on each side of the gully. From this fact it is evident that the gully must have been partly formed before or at the time when the shelves were sea-beaches. I particularly observed several instances of this structure. One which struck me most was in Glen Roy, opposite a gap in the mountain which leads to Glen Fintec; here two small threads of water were united at the point where the line of shelf crossed them, and at their junction the rock was much exposed, so that any one would have supposed that the furrow in which they flowed had been entirely hollowed out by their action. But the shelf curved in a little way on each side; and, what was more curious, the apex of turf above the point of junction of the two streamlets had evidently originally formed part of the shelf. By this it was shown that the entire hollow, with the exception of the actual beds of the streams, must have existed as an indentation or little cove on the line of ancient sea-beach. It appeared to me that the extent to which the shelves entered these furrows did not bear any close relation to the power of the streamlets now flowing in them: thus on Tombhran (in front of the houses of Roy) a great gorge which is impassable, and where the rock is bare and shattered, has been deeply cut into by the winter torrents, and yet the shelves enter only a very little way on each side; whereas in other cases we find a hollow or creek of some size, but with an insignificant stream flowing in it, for instance, that opposite the gap of Glen Fintec, which has not even removed the remnants of the shelf from the head of the gully, in which it has flowed ever since the retreat of the sea.

Without entering here into a full consideration how these gullies were originally formed, and whether the indentations made in the beach at one level might not be produced downwards to another, I will only remark, that the sea in most situations certainly does alter the form of its coast, and yet that an accurate map of any shore gives a line indented in such manner, that a series of them, if placed one above and a little behind another, would produce the same kind of furrowed surface which characterizes the mountains of Lochaber, as well as most others. I will further observe, that when travelling along the shores of northern Chile and Peru, where the alluvial action is reduced to an exceedingly small measure, and where it is not probable that within a recent period there has been any *great* change of climate, I was repeatedly much surprised at observing how absolutely similar all the minor inequalities of the surface (yet covered with beds of sea shells of existing species) were to those of countries, where almost every detail in outline is usually attributed to meteoric agency; I could perceive only one difference, namely, that the larger valleys had unusually flat bottoms. Although fully convinced of the truth of this fact, I confess I was astonished at discovering in the mountains of Scotland, which have been exposed during a vast period to the destroying action of a wet and boisterous climate, clear proofs that almost every furrow and inequality has been left nearly in the state in which we now see it*, by the retiring waves of the sea. From the preservation of

* It is scarcely possible to convey by language any accurate idea of the kind of inequalities which, from the

some of these beaches, one can point to the very spot, and declare so much was removed when the sea stood there, and so much since by the running streams of fresh water.

It may be asked, has the present alluvial action done nothing here? Something it assuredly has done, but I repeat, comparatively nothing to that which was effected before the sea retreated. In Chile I concluded that the action of the more rapid rivers and torrents was chiefly confined to removing the littoral and sublittoral deposits left by the arms of the sea; and secondarily in cutting, as soon as the upper beds were removed, a wall-sided gorge through the solid rock. It appeared, that as long as the river had its passage through the water-worn materials, from the great facility with which it changed its course, its bed was broad, but as soon as it reached the solid strata it became exceedingly narrow. These conclusions are in strict conformity with what I observed in the glens of Lochaber. Of the small amount of corrosion effected since the sea stood at a level of the upper shelves, there are some curious instances. Sir LAUDER DICK, in describing in detail the head of Glen Gluoy*, concludes that the river has worn there, during the immense period which must have elapsed since the water (of the sea) retired from the 1278-feet shelf, a remarkable chasm, between fifty and sixty feet in depth, but only a *few feet* wide. The stream in the northern arm of Glen Turet has cut for itself a passage in the solid rock in only a part of the valley, between the middle and the 972-feet shelf. In Upper Glen Roy the southern stream falls into the plain by a cascade, to the upper edge of which on each side the 1226 shelf approaches close. I did not ascend the spot, but as far as I could judge, the water has not cut back more than at most a few yards, into the rock over which it falls. Other similar instances might be adduced. Although none of these streams form great bodies of water, yet when flooded by the winter rains they cannot be inconsiderable; and their action has been prolonged for so vast a period, that the geographical features, together probably with the climate of the country,

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shelves passing over them and into the intervening hollows, we know were so left by the sea. I hope any one who feels interested on this subject, will carefully examine the plates accompanying Sir LAUDER DICK's paper (Edinburgh Transactions, vol. ix.), and especially Plate IV. The shelves on the left side (looking up the glen) bend into all the principal gullies; and on the right side, directly in front of the foreground, by looking close at the plate they will be seen to curve a little way into each of the perpendicular furrows (some thinning out and others commencing), the bottoms of which have evidently been much deepened by the descending streamlets. The idea given by these plates of the state of surface in these mountains, and of the manner in which the shelves bend round the headlands and enter the gullies, appears to me exceedingly faithful; although the glen itself, as represented, is too narrow and profound, and the sides much too steep. To view this Plate is a lesson full of instruction to the geologist, for he will scarcely fail to be astonished when he sees that the drawing is characteristic of any ordinary valley in a mountainous country, and at the same time to find himself compelled to admit, that even the little furrows, which it might be thought had been formed but yesterday, must have owed their origin, at least in great part, to the successive coves or indentations, continued one below another on ancient sea-beaches.

* Edinburgh Transactions, vol. ix. p. 26.

have been greatly changed. The rocky crests of the mountains no doubt have suffered from the weather; but the perfection of the shelves over spaces many hundred yards in length, and in the case of Glen Roy (where the three shelves occur) of some hundred feet in vertical height, clearly proves that as the sea left the greater part of the surface, so does it now remain. Amongst mountains the bursting of temporary lakes may sweep away or accumulate vast quantities of rubbish in the valleys; earthquakes may hurl down piles of fragments; and torrents during the lapse of ages, or under favourable conditions (such as the descent of many pebbles), may excavate a gorge of almost any depth, but which, as far as it is possible to judge, will always be narrow and steep-sided. All this must often have happened, and will so again; but the glens of Lochaber plainly show that the effects of ordinary alluvial action is exceedingly small, far smaller than any one would have anticipated. And as their outline does not differ in any marked degree from that of all other valleys, this conclusion may be extended to other cases.

In Glen Roy, where the three shelves can be seen near each other, little or no difference can be perceived in their state of preservation; indeed the upper one, I think, is more perfect than the one below it. From this fact an argument has been advanced by Dr. Macculloch, that no long interval of time could have elapsed between their formation. But this view is quite inadmissible; either the worn and deeply notched rock of the shelves on Tombhran, or the buttresses on the middle shelf (as at the head of Lower Glen Roy), which are composed of large masses of well-rounded shingle, is sufficient, without considering the *intermediate shelf* and other appearances, to *prove* that the water must have remained at levels intermediate between the highest and the 972-feet shelf for very long periods. Hence the alternative is obvious, and is in direct accordance with what has already been advanced, namely, that the ordinary alluvial action is so exceedingly small, that whether the surface has been exposed during one, two or more whole epochs, no sensible difference can be perceived in the state of its conservation.

Of the many remarkable features in the geology of this district, few, perhaps, are more remarkable than this perfect preservation of its surface. We have a mound composed of soft materials so small, that it oftentimes cannot be distinguished, by a person standing on it, from the adjoining slope, but which it is not probable, from the structure of the mountains, was ever much larger; and yet this very mound, when viewed from a distance, will be seen to extend for many hundred yards, even miles, continuous and perfect, with the exception, perhaps, of a few small breaks, where some streamlet descends. On these same mounds we can sometimes distinguish those fragments which have been washed by the little waves of the ancient waters, from others which have since fallen; and at Loch Treig, at the height of 972 feet above the sea, the tide-scooped rocks appear as if scarcely a century had elapsed since they were washed by the ripple of the eddying currents. The preservation of the druidical mounds in Britain has often been adduced as a circumstance worthy of attention;

but here during a period which cannot be reckoned by thousands of years, but only by those great revolutions of nature which are the effects of slow and scarcely sensible changes, works smaller than those ancient ones dedicated to superstition, retain each outline nearly as perfect as when first formed by the hand of nature.

These facts are interesting under another point of view, for they prove to us that we may trust the plain inference of our experiences. Although we see* the stone of many ancient buildings decaying and crumbling away, yet we know that others, as the obelisks of Egypt, have lasted more than three thousand years, with the hieroglyphics nearly perfect on them: now we cannot see any reason why their general outline, even in points of detail, should not last a hundred times three thousand years. Again, although we might expect the crest of a mountain range to be shattered, and the bed of a torrent to be worn down more or less deeply, yet if we look at a convex slope of soil clothed with turf, and drained on each side by rivulets, we can see no reason, as long as the vegetation is persistent, why such a slope (with the exception of any spot where a waterspout might burst, or a stroke of lightning fall) should not last for as many thousand centuries as the obelisks of Egypt shall remain entire. Of the justice of these inferences, conclusive evidence is afforded by the state in which we now see the mountains of Lochaber,—a state of which we approximately know the high antiquity.

Section IX.—*On the horizontality of the shelves, and on the equable action of the elevatory forces.*

Sir Lauder Dick, with Mr. Maclean's assistance, seems to have determined within very small limits the absolute horizontality of the several shelves. A delicate eighteen-inch levelling instrument, made by Jones, was employed. Sir Lauder says†, "Directing the object-glass of the instrument to the nearer, and immediately opposite corresponding line of shelf, it applied all along most accurately to the horizontal hair; but when pointed to those further off (some of which were perhaps five or six miles distant), they appeared to sink sensibly below the hair, and this in proportion to their distance from the point where we stood; but they were nowhere observed to do so in a greater ratio than the allowance for the curvature of the earth at such rectilineal distances demanded. And, what was in our opinion most conclusive, when the telescope was pointed to, and made to traverse along any particular portion, which, from being directly opposite to the eye, might have been presumed to be nearly equidistant in all its parts, it was found to preserve an uniform relation to the horizontal hair." The same results were obtained in other instances; but yet the angle of depression of the distant shelves does not appear to have been actually measured, and its correspondence with the curve of the earth calculated. But it is quite certain that if any

* Consult Professor Phillips's interesting paper on this subject. Geological Proceedings, vol. i. p. 323. April, 1831.

† Edinburgh Transactions, vol. ix. p. 8.

difference from that curve exists, it must be very small*. Here then is a case which supports apparently with more weight than perhaps any one hitherto advanced, the doctrine that the land is the stationary element in these changes of level, and the ocean the fluctuating one; for it may well be asked, can we suppose that a whole country shall have been lifted up without the smallest ascertained flexure of the ancient coast lines? Without reverting to the argument of the movements now in progress, some upwards and some downwards, or to the difficulty of imagining a receptacle for a stratum of water, nearly 1300 feet thick, concentric with the globe, I will consider the phenomenon in another point of view. It appears from the facts given by Mr. LYELL in his Principles of Geology†, and in the Philosophical Transactions‡, that a large territory in Sweden is now rising at the rate of three feet in a century; and that the area affected reaches from Gottenburgh to Torneo, and thence to North Cape (a distance of 1000 geographical miles), although the rate of elevation increases as we proceed northward. We may therefore safely conclude, that large spaces in Scandinavia have been elevated so equably, that at points several miles, if not leagues apart, the difference of elevation at the close of the past century, did not amount to one foot. In South America the whole coast of Chile has been elevated within the recent period; and during the great convulsions which affect that country, large spaces have been uplifted nearly to the same amount, although some parts a few feet more than others. On the eastern side of the same continent, the land has also risen within the same period, and as earthquakes are unknown there, the change probably has been, as in Sweden, so slow as to be insensible at any one time. On that side the traveller may ride for many hundred miles over plains, scarcely broken by a single undulation, and where the strata and surface are almost absolutely level: no one would there for one moment imagine that the elevatory forces had acted unequally, but rather he is astonished that the bottom of any sea or estuary should have been so uniform, as must have been that of which the plains of La Plata not long since formed the bed.

If then great plains and mountainous countries can be raised within such small limits of absolute horizontality, as undoubtedly has happened in the above cases, shall we, who are wholly ignorant of the mechanism of these movements, be justified in rejecting the plainest analogies, in supposing difficulties little short of physical impossibilities, and in believing that the reverse of what is ascertained in other cases has taken place in Lochaber, and all simply because the change of level has been

* I may here remark, that the equal elevation of the west coast of Scotland, and indeed of the whole British Islands and other parts of Europe, may be inferred from the facts collected by Mr. SMITH in his paper in the Edinburgh New Philosophical Journal. This author says (vol. xxv. p. 388.), "The great terrace (known to be of marine origin from the presence of organic remains), the base of which seems *very generally* to be between *thirty and forty feet* above the sea, forms a marked feature in the scenery of the west of Scotland."

† Book II. chap. xvii. On the gradual rise of the land in Sweden.

‡ Transactions of the Royal Society, Part I. 1835, p. 33.

more equable, than we in our ignorance could have anticipated? Every one, I think, who will attentively consider the above facts, will answer with me in the negative, marvellous though the fact be, that the beaches of Lochaber, raised on high so many hundred feet, should still follow the curvature of their ancient waters. On the contrary, a most important geological fact is established; namely that an area (twenty miles in length and eighteen broad, and perhaps more, if the shelves on the banks of the Spey be included in it) has been raised 1278 feet above the level of the sea, so equably, that no deviation from the true curvature of the earth can be discovered by the ordinary means of leveling*.

Section X.—*Speculations on the action of the elevatory forces, and conclusion.*

If we choose to enter on speculative grounds and to reflect on the secondary means which have caused these equable movements, two solutions occur. But first I must remark that the crust of the earth seems to yield easily to the forces which have acted on it from below; when we observe a brick-wall dashed to pieces by a cannon ball, or a pane of glass by a small stone, we say that both are fragile and yield easily; so when we examine the earth and find it fissured and refissured, one fragment let down and another raised high up (as we know to be the case where extensive sections have been obtained, as in our coal-pits or metalliferous districts), we must certainly admit, that the force which has broken up the crust in vertical planes relatively nearer to each other, compared with its thickness, than in the fissured pane of glass, easily overcame the resistance offered to it, however absolutely great that may have been. This same conclusion is forced on us, when we reflect that the very cause of the trembling of the ground in earthquakes seems due to the rending of the strata; and that earthquakes in many countries are of such frequent occurrence, that probably this hour will scarcely elapse without the crust somewhere yielding. If indeed the crust did not yield readily, partial elevations could not be so gradual as they are known to be, but they would assume the character of explosions. That there has been some real connexion in certain cases† between that state of the weather which is accompanied by a low barometer and the occurrence of earthquakes, can, I think, hardly be doubted; if we admit Mr. P. Scrope's explanation of this, that the diminution of atmospheric pressure (equal in some cases to an inch and half of mercury, spread over a very large area) determines the particular time at which the earthquake occurs, the force and tension being before almost balanced, we may be said to possess a rude measure of the force requisite in that area to overcome the coherence of the parts, as existing in the intervals of the recurrent earthquakes. If then the mo-

* Considering the great importance of this conclusion, and the many points of interest connected with the subject of the 'parallel roads,' it is greatly to be desired that the admirable opportunity for a close examination, afforded by the intended Ordnance Survey, will be taken advantage of by the gentlemen, so well qualified for the task, who conduct it.

† In my Journal during the voyage of the Beagle, I have mentioned (p. 431 and 432.) some instances of this.

tive force acts so gradually that the earth's crust can acquire that degree of tension, which causes large portions of it to yield readily to a very slight additional impulse; and if, as we know undoubtedly to be the case, the crust has yielded in innumerable vertical planes, intersecting each other like a net-work, and running parallel to each other at very short distances, we are compelled to admit that the equable elevation of so large an extent of country as Lochaber, must have resulted from the equable action of the elevatory forces, and not from the cohesion of its parts.

Bearing this in mind, the most obvious solution, but I very much doubt whether the correct one, is, that no force excepting the uniform expansion of solid matter from heat, could raise so equably the surface of a great *fragile* mass, as the district of Lochaber must be considered. I doubt this solution, first, because a very great expansion is necessary, especially if we include in these movements the elevation of the erratic blocks, now lying more than 2200 feet above the sea. Secondly, because the movements appear to have been of the same kind as those in the not distant country of Sweden; and there it has been shown by Mr. Lyell, that near Stockholm an alternate movement of more than sixty feet has taken place within the human period; and one is strongly tempted to believe that there is some relative connexion between the areas in Northern Europe which are rising and those which are quietly subsiding. These facts to be explicable on the theory of expansion, require, as it appears to me, far too capricious an action, in so *slowly* and far-pervading an influence as heat, to be admitted; whilst on the supposition of mechanical displacement such difficulties are not presented. Thirdly, because (and it is my chief reason for rejecting the agency of expansion by itself) the movements appear to have been of the same order with those now in progress in South America; and in that country the elevation of certain wide areas, as I endeavoured to show in a paper lately (March 7, 1838) read before the Geological Society, cannot be attributed to any other cause than an actual *movement* in the subterranean expanse of molten rock: to speak only for example sake, such as would result from a change in position of those inequalities in the ellipticity of the earth's surface, which seem indicated by the measurements of arcs of meridians. It may also be inferred, from the facts given in that paper, that the fluidity of the nucleus must be tolerably perfect. In the volcano, even the lava which is propelled to the summit of a mountain, far beyond the subterranean isothermal line of melted rock, and poured out on the surface, is oftentimes so fluid, that it runs into thin sheets like molten metal. Also at the junction of the plutonic with the metamorphic formations, we see tortuous thread-like veins branching from the former into the latter, which could only have been injected when quite liquid. Here the rock has been melted at a great depth under an enormous pressure, and yet the fluidity must have been very perfect: such plutonic rocks moreover form the beds on which all others rest. Considering these latter facts, together with the inferences deduced from the phenomena observed in South America, it may be granted as not improbable in any high degree, that this part of Scotland when it was upraised rested

on matter possessed of considerable fluidity, which underwent a slow change of form. If this be granted, there is no great difficulty in conceiving that the surface of the interior molten matter might retain that degree of curvature proper to it, as the resultant of the unknown force with that of gravity and the centrifugal impulse. Moreover, as we must conclude from what we now see going on in South America and in Scandinavia, that the area affected was large, the difference between the amount of curvature of the fluid nucleus after the rise in that part of one or two thousand feet, would be exceedingly small, and its outline scarcely distinguishable from that of the ocean, and certainly not from that of a sea affected by various tides in confined channels, which in the case of Glen Roy affords the only standard of comparison. We may almost venture to say, that as the packed ice on the Polar Sea, with its hummocks and wide floes, rises over the tidal wave, so did the earth's crust with its mountains and plains rise on the convex surfaces of molten rock, under the influence of the great secular changes then in progress.

After these considerations I am far from thinking it an overwhelming difficulty, that the curvature of the shelves of Glen Roy over a space of four, or five, or perhaps even twenty miles should appear to be the same with that of the surface of the ocean, within that limit of accuracy which the nature of the case renders possible. On the contrary, I deduce from their curvature, first, that the district of Lochaber formed only a small part of the area affected; secondly, a confirmation of the view, which I deduced from the phenomena observed in South America, that the motive power in such cases is a slight additional convexity slowly added to the fluid nucleus; and thirdly, this additional fact, that we thus obtain some measure of the degree of homogeneous fluidity of the subterranean matter beneath a large area, namely, that its particles, when acted on by a disturbing force, arrange themselves in obedience to the law of gravity. And although we arrive at this conclusion with some surprise, when relating to the abysses of the nether regions, we see it habitually verified in volcanic countries, where a torrent of lava, checked by some obstacle, has expanded into a level sheet.

Mr. Lyell, in his Principles of Geology*, quotes a passage from Sir John Herschel's Astronomy†, to show that whatever may have been the original figure of the earth, the wearing down of the solid matter and its redeposition at the bottom of the sea, must tend continually to change the *actual* figure of the earth, as Playfair‡ expresses it, into the *statical* one: he then adds, "that the same remark applies to every stream of lava flowing on the surface, and if the volcanic action should extend to great depths, so as to melt one after another different parts of the earth, the whole interior might at length be remodeled under the influence of similar changes, due to causes which may all be operating at this moment." Now if it be granted that the curvature

* Principles of Geology, Book II. chap. xviii. p. 311. 5th edit.

† Cabinet Cyclopedia. Astronomy, p. 120.

‡ Illustrations of Huttonian Theory.

of the shelves of Lochaber is due to the elevation of the district by means of a subterraneous expanse of fluid matter, the atoms of which obeyed the law of gravity, it cannot be doubted they would likewise obey that of the centrifugal force. Therefore, if the figure of the earth did not already very nearly approach to that of a spheroid of equilibrium, regions near the equator and others near the poles, during the changes of level now actually in progress, would be acted on by forces greatly different; and consequently as the crust does now yield (and has yielded in an infinite number of planes,) the statical form would be immediately acquired. This view is here given, because a directly opposite, and as I cannot but think incorrect one, has been advanced by PLAYFAIR*.

In concluding this paper, I will briefly indicate the chief points which receive illustration from the examination of the district of Lochaber by Sir THOMAS LAUDER DICK, Dr. MACCULLOCH, and myself. 1st. Nearly the whole of the waterworn materials in the valleys of this part of Scotland were left, as they now occur, by the slowly retiring waters of the sea; and the chief action of the rivers since that period has been to remove such deposits; and when this was effected, to excavate a wall-sided gorge in the solid rock. 2nd. During the vast period which must have elapsed since the sea stood at the level of the upper shelves, the alluvial action has been exceedingly small: steep slopes of turf over large spaces and the bare surface of rocks have been preserved even perfectly; and we see every main, as well as most of the lesser inequalities of the land, in the state in which they were then left. 3rd. The elevation of this part of Scotland from the level of the present beach to the height of *at least* 1278 feet has been extremely gradual, and was interrupted by long intervals of rest: it has taken place since the so-called "erratic block period." 4th. It is probable that the erratic blocks were transported during the quiet formation of the shelves. One was observed to occur at an altitude of 2200 feet above the level of the sea. 5th. The extraordinary fact that a large country has been elevated to a great height so equably, that the ancient beach-lines retain the same, or nearly the same curvature, which they had when bounding the convex surface of the ancient waters. Lastly. The inferences from this head, supported by other cases, namely, that a large area must have been upraised, and that this was effected by a slight change in the convex form of the fluid matter on which the crust rests; and, therefore, that the fluidity is sufficiently perfect to allow of the atoms moving in obedience to the law of gravity, and consequently of the effects of that law modified by the centrifugal impulse. Hence, that even the disturbing forces do not tend to give to the earth a figure widely different from that of a spheroid in equilibrium.

POSTSCRIPT.

I am much indebted to my friend Mr. ALBERT WAY for his kindness in lending me the drawing, from which the accompanying lithographic sketch has been taken. It very faithfully represents the general appearance of Glen Roy.

* Illustrations of the Huttonian Theory, p. 488.

V. *An Account of the Fall of a Meteoric Stone in the Cold Bokkeveld, Cape of Good Hope. By* Thomas Maclear, *Esq. F.R.S. &c., in a Letter to Sir* John F. W. Herschel, *Bart. V.P.R.S. &c. &c. Communicated by Sir* J. F. W. Herschel.

Received March 7,—Read March 21, 1839.

Royal Observatory, Cape of Good Hope,
November 24, 1838.

Dear Sir John,

A METEOR exploded on the 13th of October in the Cold Bokkeveld, with a noise so loud as to be heard over an area of more than seventy miles in diameter, in broad daylight, about half-past nine in the morning. It was seen traversing the atmosphere north-east of the point where it exploded sixty miles, of a silvery hue, the air at the time calm, hot, and sultry. The barometer chanced to be observed at Worcester, where the air was also calm and hot. It stood at the lowest point of its range, but, from the construction of the instrument, that point cannot be noted in inches unless by comparison with another, which I will endeavour to have done the first opportunity.

The explosion was accompanied by a noise like that from artillery, followed by the fall of pieces of matter, of which I send you the largest and best specimen I have seen, procured by Mr. Watermeyer. Portions fell or were dispersed on the ground at the distance of an hour, or five miles from each other. Some falling on hard ground were smashed; others on moist ground plunged into the earth; and I am told that one piece made a hole as broad as three feet, and sunk deep. It is stated to have been so soft as to admit of being cut with a knife where it first fell; then it hardened, but I cannot learn anything as to its temperature at that moment. If the reports are correct, I estimate the original solid mass at five cubic feet, viz. the sum of all the portions that fell to the ground.

That which I send to you is a good specimen, for the fracture is exactly similar to those I have seen that fell elsewhere, but, from being broken into small pieces, few of them have any crust or outside to show the state of fusion. This exhibits that state all over: when the two pieces are applied to each other they exactly fit, and show that it was in a state of ignition when it separated from the rest in the air.

Mr. Judge Menzies told me he was returning from circuit accompanied by Mr. George Thompson. On the morning of the 13th "he was in the bush," about sixty miles from the Bokkeveld, on his way homewards. The air was hot and calm, as preceding a thunder storm, but the clouds were not dark; on the contrary, they had

an unusual reddish tint. About half-past nine his attention was roused by something like a meteor, of a silvery colour, passing through the atmosphere, to which he directed the attention of those about him. The object moved in the direction of the Bokkeveld. He proceeded on his journey, and arrived in the evening at the place of Mr. De Toit, where he was told that a meteor had exploded in the morning, with a report as loud as "from three pieces of cannon," and that some of it fell close to the place, one nearly striking a person in a field.

Mr. Truter, Civil Commissioner of Worcester, was sitting in his office. He told me that the windows suddenly shook; immediately a rumbling noise followed, which he supposed was the precursor of an earthquake; his barometer stood at the lowest point of its range. Mrs. Truter heard a similar noise in the dwelling-house; other persons in the town were startled by the like noise; the next day he heard of the meteor in the Bokkeveld. The statement made to him by several persons is so like the statement in the inclosed letter of Mr. Watermeyer's correspondent, that it is unnecessary to repeat it.

Understanding that Mr. Watermeyer had obtained a portion of the meteorolite of considerable dimensions, I wrote to him to request a piece for you. He returned the inclosed reply, together with the whole specimen, wherein you will find that he had designed it for you. The clergyman's communication is clear and comprehensive.

On reference to the Observatory Meteorological Journal, there is nothing remarkable noted.

	h	Barom.	Out. Ther.	Wet.	Wind.	
Oct. 12.	9½	30·191	60·1	58·5	4 S.	8 blue.
	20⅔	·247	68·2	62·5	38½	6 b. cirri. Direction horiz.
13.	Noon	·242	74·2	67	3 SSW.	5 blue cirri.
	3⅓	·230	74·2	68·7	4 SSW.	6 blue cirri.

Therefore the effect did not extend so far.

You will find the Cold Bokkeveld on the map by carrying your finger along the parallel of St. Helena Bay.

Believe me, dear Sir John,

Your faithful Servant,

Thomas Maclear.

Translated Extract from a Letter of the Rev. Mr. FAHN *to Mr.* WATERMEYER, *dated Tulhagh,* 6 *Novem.* 1838.

The object of these lines is to fulfil my promise in sending to you herewith one of the stones which fell simultaneously during the atmospheric tremor in the Cold Bokkeveld, on the 13th of October. This stone was found between the estates of JACOBUS JOOSTEN and PIETER DE TOIT. Several have fallen on the place of RUDOLPH VAN HEERDEN, where one fell on the hard road, and was dashed to pieces. Another on a ploughed field sunk a few inches into the ground, and a third falling on a moist place near water, lodged itself to the depth of several feet. Some people say they observed smoke whilst these stones fell; and also that when they were picked up a smell was observable, as between sulphur and gunpowder.

The stone which you receive lay one hour distance from the place where the others were found. In the *same* direction in which the agitation was perceptible, viz. from N.W. to S.E. more stones were found. Some people saw in the same direction also a dark blue streak, which lost itself in a south-easterly direction. I have another somewhat larger stone in the Bokkeveld, which was too heavy for me to carry on horseback. If the latter one can be of service to you, I shall not fail to send it. This stone was found in *two* pieces, as it is at present.

Mr. WATERMEYER'S *Letter to* THOMAS MACLEAR, *Esq.*

Wednesday Morning,
21 Novem. 1838.

MY DEAR SIR,

I have to thank you for the favour of your note of Saturday last by Doctor KRAUSS.

As soon as I received the accompanying specimen, it was destined by me for our much-esteemed friend Sir JOHN HERSCHEL. You will therefore perhaps have the kindness to transmit it to Sir JOHN, with my sincerest regards, by some fit opportunity. I have added (in the preceding extract) whatever little information Mr. FAHN's letter contains on the subject. I shall write to Mr. FAHN by this week's post, to send me the second specimen also, of which he speaks. If there be no *immediate* opportunity of forwarding it to England, it might perhaps be proper to exhibit it first at our next Institution meeting, as I have not yet had an opportunity of showing the stone to any of its members.

Believe me, my dear Sir,

Sincerely and respectfully yours,

J. WATERMEYER.

Chemical Account of the Cold Bokkeveld Meteoric Stone. By MICHAEL FARADAY, *Esq. D.C.L. F.R.S. &c., in a Letter to Sir* J. F. W. HERSCHEL, *Bart. V.P.R.S. &c. &c. Communicated by Sir* J. F. W. HERSCHEL, *Bart.*

Received March 7,—Read March 21, 1839.

Royal Institution,
February 28, 1839.

MY DEAR SIR JOHN,

I am at last able to send you a chemical account of the meteoric stone, leaving its physical characters (except some of those which bear upon the chemical results) entirely for your observation.

The stone is soft, porous, and hygrometric. A piece of it which, at common temperature, weighed 194·4 grains, by being perfectly saturated with water under the air-pump receiver, became 202 grains, and when thoroughly dried became 182·9 grains. In its most moist condition it had a specific gravity of 2·48, which, if abstraction be made of the water in it, would give a specific gravity of 2·94 for the dry stony matter.

It has a very small degree of magnetic power, and that is irregularly dispersed in the stone.

The heat of the mouth blowpipe sends off sulphur, and softens, but does not fuse it; a higher heat, after softening it still more, makes it run into a very fluid state, the globule when cold being black and opake.

The composition of the stone may be gathered from the following analytical results, calculated for 100 parts of the stone in its natural state:

Water	6·50
Sulphur	4·24
Silica	28·90
Protoxide of iron	33·22
Magnesia	19·20
Alumina	5·22
Lime	1·64
Oxide of nickel	0·82
Oxide of chromium	0·70
Cobalt, a trace	
Soda, a trace	
	100·44

I have entered the *iron* above as protoxide, and nearly the whole of it is in that state. But there are portions, though very small, of metallic iron present. I could

not collect more than 0·06 from 100 parts of the stone. Of this the largest portion was in very fine particles, recognisable only by their magnetic properties, and the evolution of hydrogen by dilute sulphuric acid; but there was one piece of sufficient size to show the malleability, lustre, and other general properties of the metal. This metallic iron contained nickel as well as the stone generally. A part of the iron in the stone was also in the state of sulphuret, as was evident by the sulphuretted hydrogen evolved on the action of acids.

The result with regard to the *sulphur* was obtained in the form of sulphate of baryta; but though I have entered it as sulphur only in the analysis, it did not all have that state in the stone, for a part of it was there as sulphuric acid. In fact, water only, when boiled with the stone, removed small portions both of sulphate of lime and sulphate of soda; and this was the case when, on repeating the experiments, I was very careful to take parts from *the middle* of the smaller fragment, parts which had not seen the light until I broke them out. It is a question, however, whether the soda belonged to the stone when it fell, and what proportion of sulphuric acid was in it at that time; for the stone being porous and hygrometric, the water and air in it may have converted a part of the sulphur into sulphuric acid; and as to the soda, I think it must have been acquired upon the earth; for the water separated also a portion of destructible organic substance, and the larger fragment of the stone still has small particles of insoluble vegetable matter adhering to it, having the appearance of being derived from manure.

I am, my dear Sir JOHN,

Yours most faithfully,

M. FARADAY.

Sir JOHN F. W. HERSCHEL, *Bart.*
&c. &c. &c.

VI. *Fifth Letter on Voltaic Combinations, with some Account of the Effects of a large Constant Battery. Addressed to* Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By* J. Frederic Daniell, *Esq. F.R.S., Prof. Chem. in King's College, London.*

Received April 11,—Read May 30, 1839.

My dear Faraday,

In my last letter to you, which the Royal Society have done me the honour to publish in the Philosophical Transactions for 1838, I observed, that "the principal circumstance which might be supposed to limit the power of an active point within a conducting sphere, in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness." The superficial measure of the conducting sphere, and the distance of the generating metal, or the depth and resistance of the electrolyte, are, in fact, the variable conditions in a voltaic combination upon which its efficiency depends; and their relations require further investigation before we shall be able to determine what may be the proper proportions for the economical application of the power to useful purposes. I shall venture, therefore, to trouble you with the results of some further experiments upon the subject, and upon different combinations of the constant battery, before I proceed to communicate some observations upon Electrolysis, which I trust you will find not without interest, and to which, according to my plan, my attention has been lately exclusively directed.

Looking, for a moment, upon the affinity which circulates in the battery as a radiant force, it seemed desirable to ascertain what would be the result of intercepting the rays by the conducting surface nearer to their centre than in the arrangements which have been previously described, as the relation of the generating and conducting metals to each other might be thereby more clearly ascertained.

For this purpose I constructed a battery of ten cylinders of copper, nineteen inches in length by 1½ inch in diameter. As the quantity of acid and sulphate of copper which these cylinders could contain was but small, it was necessary to provide for the perpetual renovation of the charge as the zinc became dissolved and the copper precipitated. This was effected by connecting all the membranes which held the acid and the zinc rods, at their lower ends, with a pipe terminating in a stop-cock. Their upper ends were fixed in a cistern, from which they could be gradually supplied with fresh acid as the saturated acid flowed out below. All the exterior cells formed between the membranes and the copper cylinders also terminated below in a common reservoir, surrounding the former pipe, by which their exhausted contents could be

drawn off, as a fresh supply of sulphate of copper was furnished from above by an exterior cistern holding the solution. When the battery was in action, a saturated solution of sulphate of copper, thus supplied from the top, could be drawn off almost colourless at the bottom; and it remained constant for a considerable time.

The effect produced was 4½ cubic inches of mixed gases per minute, measured in the voltameter formerly described.

I compared this result with that obtained from a battery of ten cylinders, twenty inches in length and 3½ inches in diameter, which gave in the same voltameter eleven cubic inches per minute. The surfaces of the conducting cylinders were respectively 89·5 square inches and 220 square inches; and the products of gas in equal times were nearly in the same proportion as the surfaces. The zinc rods were ½ inch in diameter in both cases.

I next proceeded to compare two small hemispheres of copper with the large hemisphere of brass of 9½ inches diameter, formerly described*. They were fitted up, as before, as single circuits, with a zinc ball of one inch diameter, first placed in a membrane below the surface of the solution of copper. The measure made use of was the calorific galvanometer; the first was four inches diameter, and produced a permanent effect of 45° upon the thermometer. The second, of 2½ inches diameter, produced an effect of 29°. The effect of the large hemisphere was 90°. Here it will be observed that the action was by no means proportioned to the surfaces of the conducting hemispheres, but approximated more nearly to the simple ratios of their diameters.

Hence it would appear that the circulating force of both a simple and compound voltaic circuit increased with the surface of the conducting plate surrounding an active centre; but the experiments are not sufficient to determine the law of the increase.

I now constructed a battery of ten larger cylinders, of four inches diameter, the arrangement in everything being the same as before, and found that the action was reduced to one half, the amount of mixed gases in the voltameter per minute being only 5½ cubic inches. The experiments were repeated several times, and the action maintained for several hours, and always with consistent results. This extraordinary and sudden decline of force requires further investigation; and indeed the only conclusion which we can at present draw from the experiments which I have just detailed, but which is of considerable practical importance, is, that cylinders of 3½ inches diameter form much more effective conducting plates in a voltaic arrangement than cylinders of either greater or less diameter. It must, however, be borne in mind that this has only been proved with a series of ten cells; for it is highly probable that the limits of efficiency may change with the number of the series.

The following experiments with the different combinations which may be made with twenty cylinders, throw some light on the question of the influence of the numbers of a series, the diameter of the members of which is limited to 3½ inches. The

* Philosophical Transactions, for 1838, p. 41.

cylinders were twenty inches in height, and the zinc rods were $\frac{3}{8}$ths of an inch in diameter. The electrolyte consisted of eight parts of water and one part of oil of vitriol by measure, and in the exterior divisions of the cells was saturated with sulphate of copper. The temperature was about 65°; the duration of each experiment was one minute.

First Set of Experiments.

Number of cells .	1	2	3	4	5	10	15	20
Cubic inches of gas	0	just visible	$1\frac{1}{8}$	$3\frac{7}{8}$	6	$12\frac{1}{4}$	$15\frac{1}{3}$	$17\frac{1}{4}$

Second Set of Experiments.

	All cells direct.	1 inverted.	2 invert.	3 invert.	4 invert.	5 invert.
Cubic inches of gas .	$17\frac{1}{2}$	$15\frac{1}{2}$	$12\frac{3}{4}$	$10\frac{1}{2}$	$8\frac{1}{2}$	$5\frac{1}{2}$
	6 invert.	7 invert.	8 invert.	9 invert.		
Cubic inches of gas .	$3\frac{1}{2}$	$1\frac{5}{8}$	$1\frac{1}{8}$	just visible.		

Third Set of Experiments.

Number of double cells	5	10
Cubic inches of gas	11	20

Fourth Set of Experiments.

	5 triple cells.	5 quadruple cells.
Cubic inches of gas	$14\frac{1}{2}$	$15\frac{2}{3}$

When a series of five single cells was connected with a series of five double cells, and the same voltameter employed, the amount of gas was $14\frac{1}{2}$ cubic inches.

When two series of five double cells were each connected with the same voltameter, the amount of gas was $15\frac{2}{3}$ cubic inches. Each double series alone gave eleven cubic inches.

From these experiments it appears that the most advantageous adjustment of active force and resistance is in the series of ten single cells when they are of the diameter of $3\frac{1}{2}$ inches, and that the largest amount of work which can be derived from twenty such cells is when they are arranged in two series of ten; for

Cells.	Cubic in.	Cells.	Cubic in.	Cells.	Cubic in.
10 give	$12\frac{1}{2}$	5 give	6	4 give	$3\frac{7}{8}$
	2		4		5
20 . . .	25	20 . . .	24	20 . . .	$19\frac{3}{8}$

Twenty in single series give $17\frac{1}{2}$ cubic inches. Ten double cells give 20 cubic inches.

Cells.	Cubic inches.	Cells.	Cubic inches.
5 double give	11	5 quadruple give	$15\frac{2}{3}$
	2		
20	22	20	$15\frac{2}{3}$

I now combined in a single series a battery of seventy cells of the same dimensions, and charged in the same manner, for the purpose of observing chiefly the effects of light and heat produced by the current in a state of high intensity and constant action. The interior cells were formed of light porous earthenware, and answered their purpose perfectly, offering no more obstruction to the current than the membranes, and being much more convenient in use.

The quantity of decomposition per minute from a voltameter, with the usual charge of dilute acid, was only seventeen cubic inches; while with distilled water the passage of a current was scarcely indicated by a few bubbles of gas which rose from the electrodes.

The flame between charcoal points was of considerable volume, and formed a continuous arch when the points were separated to about three-fourths of an inch. This striking distance did not appear to be increased in a flask exhausted by the air-pump. The light and radiant heat were most intense, and proved highly injurious to the eyes of many of the party who did me the honour of assisting me in my experiments. In my own case, although protected by dark grey glasses of double thickness, a high state of inflammation was produced, which it required very active medical treatment to subdue; and, as you well know, in others, even the application of leeches was found to be necessary. The whole of my face was also scorched and inflamed as if it had been exposed for many hours to a bright midsummer sun. When reflected from an imperfect parabolic metallic mirror in a lantern, the rays were collected into a focus by a glass lens, and readily burned a hole in paper at many feet from their source. The heat was quite intolerable to the hand when held near the lantern.

Paper steeped in nitrate of silver, and afterwards dried, was speedily turned brown in the light; and when a piece of fine wire-gauze was held before it, the pattern of the latter appeared in white lines, corresponding to the parts which it protected.

The phenomenon of the transfer of the charcoal from one electrode to the other, which I believe was first remarked by Dr. Hare, was abundantly produced. The transfer took place from the zincode (or positive pole, or charcoal connected with the last copper cylinder of the battery) to the platinode (or negative pole, or charcoal connected with the last zinc rod of the battery*), and in the former a sharp, well-defined, cup-like cavity was produced, and on the latter, a corresponding protuberance or nipple. The carbon of the latter proved to be very hard, and had the rough mammillated structure of the carbon which is found coating the interior of gas retorts. When a platinum rod was substituted for the charcoal at the platinode, the transfer

* I have so strongly felt the want of some distinctive names for the two poles of the battery consistent with the principles of nomenclature which you have adopted, that I have ventured to propose those mentioned above, and have constantly used them in my lectures. The *anode* and *cathode* have relation to the surfaces of the electrolyte, and if we distinguish the electrodes themselves as the *anelectrode* and the *cathelectrode*, confusion is apt to arise; I find practically that students easily recollect that the *zincode* is that electrode which in the regular battery would be constructed of zinc, and the *platinode* of platinum.

of the charcoal from the zincode still took place, and the metal became coated with carbon, which was beautifully moulded to its extremity. When this arrangement was reversed, and the zincode was formed of platinum, and the platinode of charcoal, particles of platinum were transferred, and the charcoal became covered with distinct and numerous globules of the fused metal.

The transfer of matter of such dissimilar kinds in this definite direction, renders it probable that it is essential to the disruptive discharge, and is entirely analogous to the transfer of matter which has been observed by Fusineri in all cases of the Leyden discharge, and of the discharge of atmospheric electricity by lightning.

As connected with this subject I may recall to your recollection the attempts which we made to produce a spark before contact, and in which we reduced the distance between two platinum wires connecting the two ends of the battery, to the utmost possible degree, without success. Even when the wires were heated in the flame of a blow-pipe no discharge was established. At the suggestion of Sir John Herschel, I adjusted two brass balls connected with the two ends of the battery, within a very minute distance of each other, and, while in this situation, I passed the spark of a small Leyden jar between them, and immediately the battery current was established, and the brass balls, which were hollow, were burned. Is it not probable that the Leyden discharge in this case transferred the conducting matter which was essential to the existence of the voltaic flame, and which was afterwards supplied by its own energy?

The arch of flame between the electrodes was found to be attracted or repelled by the poles of a magnet according as one or other pole was held above or below it, as was first ascertained by Sir H. Davy; and the repulsion was at times so great as to extinguish the flame. When, according to the suggestion of Mr. Gassiot, the flame was drawn from the pole of the magnet itself included in the circuit, it rotated in a very beautiful manner*. When the zincode was connected with the marked pole, and the platinode was held over it, the rotation was from west to east, or in the contrary direction to the motion of the hands of a watch; but when the arrangement was reversed, and the zincode was connected with the unmarked pole, the rotation was reversed. The flame was also made to rotate by the induction of the magnetism of the earth upon a poker of iron held in the direction of the dip.

The experiment was again varied by leading the current through a spiral, twisted round a horse-shoe bar of soft iron, and causing the flame to rotate under the influence of its own magnetic force.

The heating power of the battery was very great, and the greater intensity of the heat on the side of the zincode than on that of the platinode extremely remarkable. Mr. Gassiot first pointed out to me, that when two stout copper wires, of $\frac{1}{3}$th of an inch in diameter, were connected with the extremities of the battery and held across each other, so that the flame passed between them, the wire at the zincode became

* This modification of Sir H. Davy's experiment was first made, as I am informed, by Mr. Sturgeon.

red hot, while the other remained comparatively cool. A bar of platinum, $\frac{1}{8}$th of an inch square, freely melted and dropped in large globules in the former situation, but showed no signs of fusion at the platinode.

When the zincode was formed of the hard carbon taken from the gas retorts, and a cavity ground in it, the most infusible metals placed in it were melted in considerable quantities.

Pure rhodium immediately ran into a perfect globule, and burned with scintillations and a blue light. The native alloy of iridium and osmium, as well as pure iridium, were also completely melted. These metals were kindly supplied to me for the experiments by Mr. Johnston.

Titanium fused instantly, and burned with scintillations very much resembling those from iron.

The native ore of platinum was completely fused; but the mass, when cold, proved to be very brittle.

After four hours constant action, the power of the battery was found to be undiminished, and the amount of the zinc consumed was very small.

In conclusion, I shall briefly describe the results of some experiments on the evolution of the mixed gases from water in a confined space, and under consequent high pressures, which I made from July to October 1837, and which I intended to have further extended. My objects were, to ascertain, 1st, in what manner conduction would be carried on, supposing that the tube in which the electrodes were introduced were quite filled with the electrolyte, and there were no space for the accumulation of the gases; 2ndly, whether decomposition having been effected, recombination would take place at any given pressure; and, 3rdly, whether any reaction on the current-force of the battery would arise from the additional mechanical force which it would have to overcome.

The first apparatus which I made use of was a stout glass tube, into the lower end of which a platinum wire was inserted to form an electrode. This end was hermetically closed, and the upper end ground and fitted with a platinum valve pressed upon by a lever, which could be loaded with weights to any required amount. From this valve a wire projected into the tube to form the other electrode. The tube was accurately filled with the standard dilute acid, and placed in the battery circuit with a voltameter, by which the rate of work and the quantity of the gases disengaged could be ascertained. The battery made use of consisted of ten large cells with the usual charge. Before pressure was applied, the rate of work was always ascertained with the tube and voltameter in their places. I tried many experiments with this arrangement; but it will only be necessary to describe the general results.

The pressure was carried up to 98 lbs. upon a circular area of $\frac{3}{4}$ inch diameter, the apparatus appeared to be quite tight for a long time, and bubbles of gas were evolved from the two wires when the circuit was complete. The liquid became hazy, and bubbles of gas seemed to line the tube. The stream of oxygen from the upper wire

was projected downwards into the liquid, as if with considerable force. The liquid ultimately oozed out between the edge of the tube and the valve, and the experiment was stopped. When the pressure on the valve was removed, a puff of gas took place and the liquid slowly effervesced for a considerable time, but was not projected with any violence. The compression tube felt warm to the hand but not very hot. The quantity of gas which first escaped seemed to bear but a small proportion to that which was indicated by the voltameter included in the circuit, and the rate of decomposition was not at all altered by the accumulation of the elastic force.

To carry the experiment as far as the resistance of glass could conveniently admit of, I caused a compression tube to be made of $\frac{1}{8}$th inch in thickness, of the capacity of $1\frac{3}{10}$th cubic inch. Two platinum plates were sealed into its lower end; one cubic inch of standard acid was poured into it, and it was then hermetically sealed at the top. It was placed securely in the battery circuit with a voltameter, and the progress of the experiment was watched from a safe distance.

The evolution of the gas, which was measured at short intervals, took place with perfect regularity, and did not appear to be in the slightest degree affected by the gradually increasing compression. In $4\frac{1}{2}$ minutes, when nineteen cubic inches had been collected, the compression tube burst with a loud explosion, and the fragments, which were very small, were scattered all over the laboratory.

If we were to calculate that nineteen cubic inches were compressed into the $\frac{3}{10}$ths of a cubic inch space, unoccupied by the liquid, this would be a compression of sixty-three into one, and the pressure would amount to nearly 940 lbs. upon the square inch; but if we were to reckon, as was probably the case, that two cubic inches of the gases were kept down by the solvent power of the liquid at this high pressure, then the compression would have amounted to fifty-six into one, and the pressure to 840 lbs. upon the square inch.

It is probable that the means here pointed out might be applied with advantage to the compression of some of the gases whose liquefaction you have already effected; and I purpose, when my avocations will permit, to return to the experiments with this view.

I remain, my dear FARADAY,

Your faithful friend,

J. F. DANIELL.

King's College, London,
April 9th, 1839.

VII. *On the Electrolysis of Secondary Compounds. In a Letter addressed to* Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By* J. Frederic Daniell, *Esq. F.R.S., Prof. Chem. in King's College, London.*

Received May 15,—Read June 13, 1839.

My dear Faraday,

I HAVE no doubt that you will agree with me in thinking that the decomposition of secondary compounds by the voltaic current, particularly in connexion with water, has not yet received all the attention which it deserves, and that the subject is worthy of further experimental research.

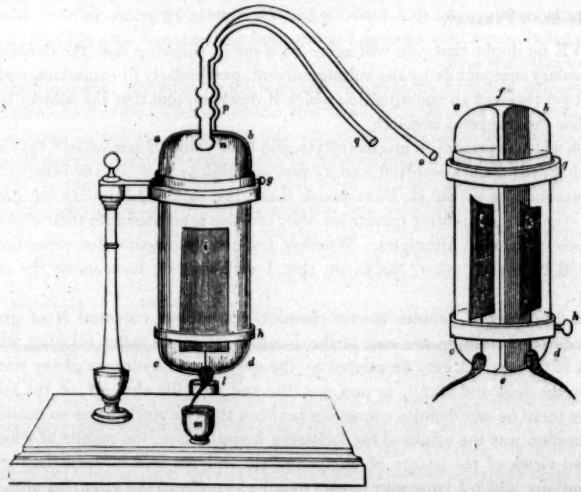
When water is present in an electrolyte, you have yourself remarked* that it is probable that it is always resolved into its first principles; and, on the other hand, the early experiments of Sir H. Davy prove that when saline substances are present in water, even in the minutest quantities, they are also separated into their elements, or into their proximate principles. Whether these simultaneous decompositions bear any relation to each other, has never, that I am aware of, been made the object of inquiry.

Your discovery of definite electro-chemical action has rendered it of great importance to ascertain, in the case of the decomposition of a saline solution, what proportion of the current may be carried by the oxygen and hydrogen of the water, and what by the acid and alkali, or non-metallic and metallic element, of the salt; and whether there be any definite connexion between the two electrolytes so decomposed. This question was the origin of the following investigation, the results of which have disclosed views of the nature of compound electrolytes and of secondary chemical combinations, which I trust may not be found unworthy of the attention of the Royal Society.

The power which I employed in the following experiments was that of a small constant battery of thirty cells six inches in height, with tubes of earthenware charged in the manner which I have formerly described; and I cannot but remark that without such an instrument the investigation could not have been carried on. The battery was in action almost daily for more than two months, generally from five to six hours per day, and the copper cylinders have thereby become considerably thicker and heavier. Neither the expense nor the uncertain action of batteries of the old construction would have admitted of such an use.

* Experimental Researches in Electricity, § 671.

The experimental cell which I first employed consisted of a stout glass cylinder, capable of holding about fourteen cubic inches of liquid, which was originally closed at both extremities. It was cut longitudinally into two equal parts, for the purpose of inserting between the halves a thin plate of fine porous earthenware, which, when the whole was again clamped together by brass rings with screws, divided it into two compartments. The wire of a platinum electrode, $2\frac{3}{4}$ inches in length and one inch in width, passed through the bottom of each compartment; and to the top of each was ground a bent glass tube for the purpose of collecting the gases evolved during the experiments. The following cut represents a front and side view of the apparatus.



a b c d is the glass cylinder; *e f* the porous diaphragm; *g h* the brass rings and screws by which the two halves are clamped together; *i* and *k* are the two electrodes; *l* and *m* the mercury cups by which the connexions are made with the battery; *n o* and *p q* are the bent tubes by which the gaseous products are collected.

In a preliminary trial one of the compartments was filled with distilled water, and remained for twenty-four hours without penetrating to the other in any appreciable quantity.

A mixture of sulphuric acid and water was prepared beforehand for the purpose of ascertaining in the usual way, by the alkalimeter tube, the amount of any alkaline matter which it might be employed to neutralize; and a similar solution of carbonate of soda for measuring acids. In calculating the results of the experiments, 70·8 cubic inches of mixed hydrogen and oxygen gases have been taken as equivalent to nine

grains of water, or one equivalent upon the hydrogen scale expressed in grain weights; and to facilitate computation and comparison, as well as to obtain quantities which might be certainly measured, the experiments were generally continued till the quantities of gases collected indicated a half or a whole equivalent.

Experiment 1.—The cell was charged with a solution of sulphate of soda of the specific gravity 1052, so as to cover the electrodes and to fill about half its capacity. When connexion was made with the battery it was found to conduct well; and the decomposition was allowed to proceed till twenty cubic inches of hydrogen had been collected from the platinode, and nine cubic inches of oxygen from the zincode.

The platinode solution was drawn off carefully with a glass syphon, and found to be strongly alkaline, and to contain by the alkalimeter 12 grains of free soda. The zincode solution was very acid, and neutralized carbonate of soda equivalent to 15·1 grains of sulphuric acid.

The results, therefore, of even this first experiment evidently indicate that the decomposition of an equivalent of water was accompanied by the decomposition of an exact equivalent of sulphate of soda, for the differences are but of inconsiderable amount. I shall not dwell upon the want of exact correspondence between the oxygen and hydrogen, for these are well understood; but taking the amount of the mixed gases as corrected for pressure and temperature at 28·3 cubic inches, we have the following proportions.

Cubic in.		Cubic in.		Equiv. of soda.		Soda.
70·8	:	28·3	: :	32	:	12·8
				Equiv. of sulph. acid.		Sulph. acid.
70·8	:	28·3	: :	40	:	16·1

The experimental results of 12 soda and 15·1 sulphuric acid are not more below the calculated results than might have been expected from the mode of experimenting. It must also be observed, that the level of the solution in the two divisions of the cell altered very much during the experiment; and at its termination the liquid stood 1½ inch higher at the platinode side than at the zincode.

These exact equivalent results are by themselves very remarkable; but I was now anxious to ascertain whether the power of the current were equally divided between, what had hitherto been considered to be, the true equivalent electrolytes.

Experiment 2.—The last experiment was repeated with the same solution of sulphate of soda; but a voltameter, whose electrodes were of the same dimensions as those of the experimental cell, charged with the standard mixture of sulphuric acid and water, was included in the circuit. The experiment was carried on till 70·8 cubic inches of mixed gases had been collected from the voltameter, when it was found that the hydrogen from the platinode of the experimental cell was 47·5 cubic inches, and the oxygen from the zincode 20·25 cubic inches. The former is almost exactly equal to the hydrogen indicated by the voltameter, while the latter is a little short of the equivalent proportion of oxygen. There can, however, be no doubt that the quantities

of mixed gases from the saline solution and from the dilute sulphuric acid were equal. Free acid and alkali were found at the zincode and platinode respectively, as in the first experiment, but owing to an accident were not neutralized.

Now if we regard, in the usual way, the convection of the current as effected in the voltameter by the transfer of the oxygen and hydrogen alone, we appear at first to be led to this extraordinary conclusion, namely, that the same current, which is just sufficient to separate an equivalent of oxygen from an equivalent of hydrogen in one vessel, will at the same time separate an equivalent of oxygen from hydrogen, and an equivalent of sulphuric acid from an equivalent of soda, in another vessel. The clearing up of such a result must obviously be of the first importance.

As secondary objects of interest in this experiment, it may be observed, that the temperature of the liquid in the experimental cell rose to 130° Fahr., while that of the acid in the voltameter did not exceed 67°, the quantity of the former considerably exceeding that of the latter; and that the transfer of liquid from the zincode to the platinode occurred as before, so that at the end of the experiment the level of the latter was considerably above that of the former.

The second experiment was repeated twice, and both times the disengaged acid and alkali were neutralized, and found to be in equivalent proportion to the oxygen and hydrogen given off by the electrodes of the experimental cell; which again were together equal to the mixed gases simultaneously given off by the attached voltameter. When the process was continued too long, the proportions of acid and alkali fell short of that of the gases, and that in proportion to the time of its continuance. This was doubtless owing to the gradual mixing of the liquids on the two sides of the diaphragm, and the consequent recombination of the acid and alkali. In every case, transfer of the liquid took place from the zincode to the platinode, and the temperature of the experimental cell rose far above that of the voltameter.

Experiment 3.—The experimental cell was charged with sulphate of potassa of the specific gravity 1069; and a similar cell with dilute sulphuric acid of the specific gravity 1150, coloured on the zincode side with indigo. Both were included in the circuit. After forty minutes action, the saline solution had risen on the platinode side one-fourth of an inch, and fallen to the same amount on the zincode side, while no transfer of liquid had taken place in the acid cell, nor had any colouring matter passed from the zincode side of the diaphragm to the platinode. The indigo, however, had entirely lost its blue colour and become yellow. The amount of mixed gases from both cells was nearly equal; but the oxygen from the acid fell a little short of that from the salt, in consequence, probably, of its absorption by the indigo. The acid disengaged at the zincode and the alkali at the platinode of the saline solution, were also found to be in equivalent proportions to each other and to the gases.

Experiment 4.—A solution of nitrate of potassa of the specific gravity 1117 was substituted for the solution of sulphate of potassa in the last experiment; five cubic inches of oxygen were given off by the zincode, and 4·3 cubic inches of hydrogen

only from the platinode. Upon opening the apparatus, a strong smell of ammonia was evolved from the platinode side; evidently indicating a secondary action of the hydrogen upon the nitric acid of the nitre, and accounting for its deficiency. After driving off the ammonia by heat the solution was still alkaline, and by neutralization indicated 13·5 grains of potassa. The neutralization of the acid on the zincode side indicated 17 grains of pure nitric acid. These numbers do not differ much from equivalent proportions, which, upon the assumption that the determination of the acid was correct, would be 17 nitric acid, and 15·1 potassa; or, preferring the determination of the alkali, would be 15 nitric acid, and 13·5 potassa. The quantity of water simultaneously decomposed we can only, in this instance, estimate from the quantity of oxygen collected, which, as we have seen, generally falls below its due proportion; but still it comes near enough to the equivalent proportion to assure us that this would have been the correct determination for

Cubic inches.		Equivalent of nitric acid.		Cubic inches.		Nitric acid.
70·8	:	54	: :	15·0	:	14·2

In this experiment, less of the solution was carried from the zincode to the platinode, and the difference of the level did not exceed $\frac{1}{16}$ths of an inch.

Experiment 5.—The experiment was repeated with the substitution of a solution of phosphate of soda of the specific gravity 1057 for the nitrate of potassa, and the results left no doubt that the simultaneous decompositions of the salt and the water were in equivalent proportions as before. In this experiment, a larger quantity of the solution was carried from the zincode to the platinode than in any of the preceding instances, and the difference of the level on the two sides of the diaphragm amounted to two inches.

Before I proceed with the principal object of this inquiry, I will make one or two remarks upon this extraordinary transfer of matter from one electrode to the other without the decomposition of the transported compound. It was first observed by Mr. Porret*, who found it to take place in a glass cell divided into two by a diaphragm of bladder. Into each compartment, filled with water, he plunged a platinum plate and connected the two with the two extremities of a voltaic battery of eighty couples, and nearly the whole of the liquid in the positive cell was carried into the negative cell. It was afterwards found that this phenomenon did not take place when the conducting power of the water was improved by the addition of sulphuric acid.

M. Becquerel has also shown† that when finely-divided clay is placed about the water at the zincode in a tube separated from the platinode by a porous diaphragm, the particles of clay are carried forward by the current which is established. This transfer he has also observed to take place only when the water conducts badly.

I charged the experimental cell with the porous diaphragm with distilled water, and found, with the battery of thirty cells, that a few bubbles of gas formed upon the

* Annals of Philosophy, July 1816.

† Traité de l'Electricité, tom. iii. p. 102. par M. Becquerel.

electrodes, but that none was disengaged: in forty minutes, however, the liquid in the platinode cell stood half an inch higher than that in the zincode. This experiment was repeated, a little fresh precipitated alumina having been diffused in the water on the zincode side: a portion of this finely-divided solid matter evidently passed with the water which was transferred to the platinode side.

When the water was saturated with boracic acid, its conducting power was a little improved; but still no measurable quantity of gas was given off, and in forty minutes the difference of level in the two compartments of the cell was $\frac{1}{10}$ths of an inch. When the cell was charged with a mixture of eight parts water and one part sulphuric acid, no change of level took place on the two sides.

Notwithstanding the good conducting power, however, of the saline solutions, we have seen that this passage of liquid from the zincode to the platinode occurs with them even to a greater extent than with pure water, and the different species of salts seem to be acted upon in different degrees.

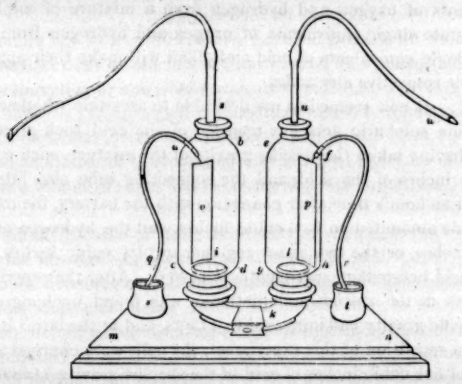
To ascertain whether, during this process of convection, any separation took place of the salt from its solvent, I took the specific gravity of the solutions both before and after the process, and found the differences so trifling as evidently not to arise from this cause. Neither does this large transfer of matter, which is always in the same direction, seem to interfere with the definite electrolytic effect of the current by which it is produced. This will be shown still more strikingly from the subsequent experiments. Whatever may be its proximate cause, the phenomenon appears to be analogous to the transfer of good conducting matter which takes place from the zincode to the platinode during the disruptive discharge of the battery in air, which I referred to in my last communication*, and in which I pointed out to you that the platinum of the zincode was carried forward and deposited upon the charcoal of the platinode. I am also disposed to think that it is quite distinct from the process which M. Dutrochet has named *endosmose* and *exosmose*, which may be satisfactorily explained by the force of heterogeneous adhesion, without reference to electrical currents.

I have already stated that the products of electrolyzation cannot be kept long separate in the cell which I have described, on account of the ultimate mixture of the liquids on the platinode side of the diaphragm; I was, therefore, led to construct an apparatus which answers this purpose much more perfectly, and which, for distinction, I shall call the double diaphragm cell. It consists of two cells formed of two glass cylinders, with collars at their lower ends, fitted by grinding to a stout glass tube bent into the form of the letter U, and firmly fixed on a wooden foot. The ends of this piece project a little into the interior of the two cylinders, the upper extremities of which are furnished with bent tubes for the collection of gases. A stout piece of platinum wire is ground into the upper part of each cell, to which an electrode of platinum or any other metal can be screwed on the inside, as occasion may require: the wires pass down upon the outside, and terminate in two mercury cups, by

* Present Volume, p. 92, *et seq.*

which connexion can be made, at pleasure, with the battery. Each cell will hold about seven cubic inches of liquid, and the connecting tube two cubic inches. When the cell is charged, the connecting tube is filled with the liquid, and a piece of fine bladder tied over each end, so as perfectly to exclude the air. The bladders are firmly confined to their places by means of circular grooves ground round the ends of the glass tube. The cylinders are then carefully fitted to their places, and filled with the proper quantities of the solutions to be acted upon, and after the operation their contents are easily decanted, by means of a small glass syphon.

The apparatus is delineated in the following cut.



a b c d and *e f g h* represent the two glass cells; *i k l* is the bent glass tube fixed in the wooden stand *m n*; *o* and *p* are the electrodes connected with the mercury cups, *s* and *t*, by the wires *a q* and *f r*; *s t* and *u w* are the bent tubes for the collection of the gases evolved.

The current when transmitted by this double cell was, of course, much more retarded than by the single cell, on account of the greater distance of the electrodes, but it answered its intended purpose of stopping the transfer of the liquid even in the case of saline solutions, and there was still conducting power enough to render it perfectly effective. The bladders, however, of the connecting tube generally became a little cupped; indicating a pressure from without, owing to a transfer of a small portion of the liquid within.

Experiment 6.—The two cells were charged each with 4½ cubic inches of a saturated solution of sulphate of soda, and the connecting tube was also filled with the same, and in 2½ hours the hydrogen evolved from the platinode was twenty-four cubic inches, and the oxygen from the zincode ten cubic inches, making a total of the mixed gases of thirty-four cubic inches; or, making allowance for the deficiency of oxygen, about half an equivalent. After the experiment, the quantities of liquid in the two

cells were found unchanged. The zincode solution indicated by saturation the existence of 19·2 grains of free sulphuric acid, and the platinode solution 13·5 grains of free soda, to which must be added two grains of soda in the connecting tube; making a total of 15·5 grains of soda. These numbers are almost identical with the half equivalents of sulphuric acid and soda.

This and similar experiments were frequently repeated with perfectly consistent results; but it will be unnecessary to occupy your time with their details, as I consider that I have already adduced sufficient evidence to prove, that, in the electrolysis of a solution of a neutral salt in water, a current which is just sufficient to separate single equivalents of oxygen and hydrogen from a mixture of sulphuric acid and water will separate single equivalents of oxygen and hydrogen from the saline solution, while single equivalents of acid and alkali will make their appearance at the same time at the respective electrodes.

Experiment 7.—It now seemed to me desirable to ascertain whether in the electrolysis of the dilute sulphuric acid any transfer of the acid took place; and for this purpose, after having taken the specific gravity of the mixture, each cell was charged with five cubic inches of the acid, and the connecting tube also filled with it. In little more than an hour's time after connexion with the battery, the oxygen collected from the zincode amounted to 23·6 cubic inches, and the hydrogen at the platinode to 47·5 cubic inches, or the two gases together to 71·1 cubic inches. The specific gravity of the acid before the experiment was 1126·6. After the experiment, the bulk of the acid both at the zincode and platinode was found unchanged; but, at the former, the specific gravity had increased to 1130·3, and at the latter it had decreased to 1120·3. In a repetition of this experiment, the cells were charged with four cubic inches instead of five cubic inches of acid of the specific gravity 1128·0, and after the collection of the same quantity of the mixed gases, the specific gravity of the acid at the zincode was found increased to 1141·0, and that of the platinode decreased to 1123·0.

From these experiments we learn, that during the electrolysis of an equivalent of water, a portion of acid passed from the platinode to the zincode, and possibly of water from the zincode to the platinode; and, from the greater increase of specific gravity in the smaller quantity of the mixture than in the larger, that the quantities were nearly equal in the two experiments.

But this method did not appear to me capable of determining the point with that precision which its importance rendered desirable; I therefore endeavoured to ascertain the quantity of acid transferred by actually weighing the cells before and after the process.

Experiment 8.—I made use of an apparatus in which the connecting tube covered with its two diaphragms was placed at the top of the two cells, its ends dipping below the surface of the liquid which they contained. The quantity of water decomposed was ascertained by a voltameter included in the circuit. The mixed gases col-

lected amounted to eighteen cubic inches. The weights of the different parts of the apparatus charged with the dilute acid before and after the experiment were as follows:

	Before experiment.	After experiment.	
	grs.	grs.	gr.
Connecting tube . . .	539·2	537·3	1·9 loss.
Platinode cell	2687·8	2685·2	2·6 loss.
Zincode cell	2631·5	2634·0	2·5 gain.
	5858·5	5856·5	
Total loss		2	
	5858·5	5858·5	

Now if we multiply the amount of the gases by four, the product will be seventy-two, or nearly an equivalent of water decomposed. The total loss, two grains multiplied by four, will be eight grains, which is sufficiently near the equivalent weight of water to lead to the conclusion that the difference arises from errors in experiment: and 2·5 grains, the quantity of acid transferred, multiplied by four, will give ten grains as the amount of sulphuric acid transferred during the electrolyses of one equivalent of water.

This experiment was several times repeated, and the result was always a small gain of weight in the zincode cell, and a loss in the platinode; but I found that I could not rely upon this method of operating for the accurate determination of quantities which it was my object to obtain.

Experiment 9.—I therefore returned to the double diaphragm cell, which I charged with dilute sulphuric acid of different degrees of strength, and having ascertained the quantity of crystallized carbonate of soda which each mixture would neutralize before the experiment, I measured the neutralizing power of each cell after it had been exposed to the battery current. The process was continued in most cases till 70·8 cubic inches of the mixed gases had been collected; and, for the sake of comparison, in the following table, the results have been all brought up to one equivalent by calculation.

Table showing the saturating power of dilute sulphuric acid at the two electrodes for one equivalent of water decomposed.

	1 Acid. 8 Water.	1 Acid. 16 Water.	1 Acid. 32 Water.	1 Acid. 64 Water.	1 Acid. 128 Water.	1 Acid. 8 Water.
Platinode ..	−24	−29	−32	−37	−39	−35
Zincode	+20	+35	+31	+37	+36·5	+36

Now, if we reject the results of the first experiment (for which, however, I have been unable to discover any reason except their inconsistency with the others,) all the rest agree very closely together, notwithstanding the great differences in the mixtures of acid and water from which they were obtained. The mean result also accords

very well with the determination at which we had arrived, in Experiment 8, of the transfer of ten grains of sulphuric acid from the platinode to the zincode for every nine grains of water decomposed for

Equivalent of crystallized carbonate of soda.		Equivalent of sulphuric acid.		Carbonate of soda.		Sulphuric acid.
144	:	40	: :	36	:	10

The experiments, however, I conceive, are not sufficient to determine whether the differences of neutralizing power are owing solely to the transfer of acid from the platinode to the zincode, or whether they arise partly from such a transfer, and partly from a simultaneous passage of water from the zincode to the platinode. I have been unable as yet to contrive any experiment to determine this point, which is one of considerable interest.

Experiment 10.—Wishing now to vary the conditions of the last experiment, and at the same time to ascertain whether the same transfer of acid takes place in the active cells of the battery, where the oxygen of the decomposed water is taken up by zinc instead of being given off from a platinum surface, I substituted an amalgamated zinc plate for the platinum in the zincode cell. Its weight before the experiment was 442·5 grains. Each cell was charged with 1123 grains of dilute acid, containing, by calculation from its neutralizing power, 158·8 grains of anhydrous acid. The action of the battery was continued till 23·5 cubic inches of hydrogen had been collected from the platinode, indicating the decomposition of half an equivalent of water. After the experiment, the zinc electrode weighed 426·5 grains, the deficiency being sixteen grains, or half an equivalent of zinc.

The saturation of the liquid at the zincode now indicated 145·1 grains of acid, to which must be added twenty grains for the saturation of the oxide of the half-equivalent of zinc dissolved, making a total of 165·1 grains of acid, and showing an excess of 6·5 grains over the original quantity in the cell.

The saturation of the acid at the platinode indicated 151·8 grains, or a deficiency of seven grains.

This experiment, therefore, leads to the conclusion that about thirteen grains of acid for an equivalent of water decomposed travelled from the platinode to the zincode; but looking to the number, and indeed the uncertainty, of the processes from which the ultimate quantities were deduced, it ought not, perhaps, to be considered as affording more than a general confirmation of the results previously obtained, viz. that a fourth of an equivalent of sulphuric acid passes from the platinode to the zincode for every single equivalent of a compound which has been electrolyzed by the current.

Allow me here to recall to your recollection your own experiments, in which you compared the quantity of acid transferred from the platinode to the zincode from a mixture of sulphuric acid and water, with the quantity transferred by the same current from a solution of sulphate of soda, and found it in one instance as $\frac{1}{37}$th to $\frac{1}{10}$th,

or as 0·027 to 0·100, which very little exceeds one fourth, although from another experiment you obtained somewhat more*.

Experiment 11.—The question now arose, Does the acid, during its transfer in the case of the mixed acid and water, or do the acid and alkali in the case of the saline solution, convey any portion of the current which effects the simultaneous decomposition of the water in both instances?

To answer this question the following arrangement was made. The double cell was charged with a saturated solution of sulphate of soda, and connected with it in the circuit was placed a green-glass tube, into the bottom of which was welded a platinum wire forming the platinode to a portion of chloride of lead, which was kept fused in the tube by means of a spirit lamp. The corresponding zincode was formed of plumbago. The platinum wire before the experiment weighed 5·44 grains. The apparatus conducted well, and chlorine escaped in abundance from the plumbago electrode. When 11·7 cubic inches of hydrogen had been collected from the platinode of the double cell, the experiment was stopped and the results examined.

A well-formed button of lead was found adhering to the platinum wire; and the two together, after having been thoroughly freed from the adhering chloride, weighed 27·1 grains, making the reduced lead 21·66. This is about 4½ grains deficient of the quantity which, adopting the chemical number of lead, it ought to have been to be in proportion to the quarter-equivalent of water, which the hydrogen showed to have been decomposed. The sulphuric acid at the zincode amounted to 9·63 grains, and the soda at the platinode to 7·5 grains, both approaching, respectively, very nearly to 10 and 8, which are the exact equivalent numbers. No manner of doubt can therefore exist, that the same current which is just sufficient to resolve an equivalent of chloride of lead, which is a simple electrolyte unaffected by any associated compound, into its equivalent ions, produces the apparent phenomena of the resolution of an equivalent of water into its elements, and at the same time of an equivalent of sulphate of soda into its proximate principles.

Without this experiment, it might have been supposed that in Experiment 2. the acid, which we know must have passed from the platinode to the zincode of the voltameter at the time of the passage of the hydrogen and oxygen, carried the force which effected the decomposition of the sulphate of soda in the experimental cell; but we may now be certain that this is not the case. The last experiment was repeated with exactly similar results.

It was obviously of great moment to inquire next into the action of the voltaic current on the aqueous solutions of the chlorides, as the simpler constitution of this class of salts promised to throw some light upon the nature of the electrolysis of secondary compounds.

Experiment 12.—A plate of pure tin, whose weight was 375·8 grains, was made the zincode of the double cell, which was charged in all its parts with a strong solution of chloride of sodium. During the passage of the current, not the slightest particle

* Experimental Researches, § 529.

of gas appeared upon the tin electrode, nor was the slightest smell of chlorine evolved. The experiment was stopped when twenty-four cubic inches of hydrogen had been collected from the platinode. The tin electrode now weighed 346·1, the loss being 29·7 grains, or almost exactly half an equivalent, and corresponding to the quantity of hydrogen evolved. The platinode solution was alkaline and indicated fifteen grains of soda, to which if we add one grain for some soda in the connecting tube, the solution in which was alkaline, we shall have an exact half-equivalent of soda.

Experiment 13.—The last experiment was repeated with the addition to the circuit of a tube containing, as in Experiment 11., fused chloride of lead. The results are stated in the following table, and compared with the exact chemical equivalents:

	Experiment.	½ equivalent calculated.
Hydrogen evolved . .	12·6	11·8
Lead reduced	24·9	26·0
Tin dissolved	16·3	14·6

The solution in the platinode cell was alkaline, but its saturation was omitted.

Now the simple way of regarding the results of this experiment is, to suppose that, for an equivalent of chloride of lead electrolyzed in the first cell, an equivalent of chloride of sodium was decomposed in the second cell; the chlorine of the latter being absorbed by the tin zincode, and the sodium at the platinode reacting upon the water, and giving rise to the secondary result of an equivalent of hydrogen: upon this hypothesis, the current must have been transmitted by the chloride of sodium alone, and no water was electrolyzed.

Indeed, we must lay it down as a fundamental principle, in discussing the results of all these experiments, that the force which we have measured by its definite action at any one point of a circuit cannot perform more than an equivalent proportion of work at any other point of the same circuit: that the current which we have measured by its electrolyses of an equivalent of simple chloride of lead cannot, at the same time, be sufficient to electrolyze an equivalent of chloride of sodium and an equivalent of water at the same electrodes. The sum of the forces which held together any number of ions in a compound electrolyte could, moreover, only have been equal to the force which held together the elements of a single electrolyte, electrolyzed at the same moment in one circuit.

But how shall we apply this principle to the electrolysis of the solution of sulphate of soda and the results of Experiment 11.? Water seemed to be electrolysed; and, at the same time, the acid and alkali of the salt appeared in equivalent proportion with the oxygen and hydrogen at their respective electrodes. We cannot admit that after the decomposition of the water there was any excess of force applicable to the decomposition of the salt; but we must conclude that the only electrolyte which yielded was the sulphate of soda, the ions of which, however, were not the acid and alkali of the salt, but an *anion* composed of an equivalent of sulphur and four equivalents of oxygen and the metallic *cathion* sodium; from the former, sulphuric acid was formed at the *anode* by secondary action and the evolution of one equivalent of oxygen; and

from the latter, soda at the cathode by the secondary action of the metal and the evolution of an equivalent of hydrogen.

These electro-chemical considerations are applicable, of course, to many other saline combinations, as I shall hereafter show; and, in the experiments which I have already detailed, lead to the conclusion that, considered as electrolytes, the following change should be made in the chemical formulæ of the several salts employed.

	Chemical formula.	Electrolytic formula.
Sulphate of soda	$(S + 3\,O) + (Na + O)$	$(S + 4\,O) + Na$
Sulphate of potassa . . .	$(S + 3\,O) + (P + O)$	$(S + 4\,O) + P$
Nitrate of potassa . . .	$(N + 5\,O) + (P + O)$	$(N + 6\,O) + P$
Phosphate of soda . . .	$(P + 2\frac{1}{2}\,O) + (Na + O)$	$(P + 3\frac{1}{2}\,O) + Na$

This view leads me to a modification of the opinion which I have hitherto entertained of the decomposition of sulphate of copper in the constant battery, and the electrolysis of salts, the metallic constituent of which is incapable alone of effecting the decomposition of water at ordinary temperatures. I have always ascribed the appearance of the copper upon the platinode to the secondary action of the hydrogen evolved at that point; but the considerations which I have just submitted to you oblige me to consider it as a primary result of the electrolytic action, the electrolytic formula of sulphate of copper not being $(S + 3\,O) + (Cu + O)$, but $(S + 4\,O) + Cu$. The following experiments were made to elucidate the point still further.

Experiment 14.—The double diaphragm cell was charged at the platinode with a saturated solution of sulphate of copper; the connecting tube and the zincode cell were both charged with dilute sulphuric acid of the standard strength, and a voltameter was included in the circuit. The process was stopped when 35 cubic inches of the mixed gases had been collected. The copper which was precipitated upon the platinode weighed 15·5 grains, and the solution in the platinode cell, which was acid, indicated by neutralization with carbonate of soda, 18·8 grains of free sulphuric acid. The results all approach very nearly to exact equivalent proportions, as shown in the following Table:

	By Experiment. Cubic inches.	By Calculation. Cubic inches.
Oxygen and hydrogen	35	35·4
Precipitated copper	15·5	16
Precipitated sulphuric acid . . .	18·8	20

Experiment 15.—The last experiment was repeated with the substitution of a zincode of zinc for one of platinum: the results are shown in the following Table, and their comparison with the exact equivalent numbers.

	By Experiment. Cubic inches.	By Calculation. Cubic inches.
Oxygen and hydrogen	35	35·4
Precipitated copper	16·7	16
Dissolved zinc	16·4	16
Dissolved sulphuric acid . . .	18·8	20

The appearance of the free sulphuric acid at the platinode cell instead of the zincode is very remarkable. According to the principle which I have laid down, the following view must be taken of the results. The convection of the current in the double cell must have been effected by the electrolysis of the compound electrolyte sulphate of copper (S + 4 O) + Cu, and of the simple electrolyte water H + O, the charge being carried by one to its point of junction with the other, and there delivered up to it. If, for convenience, we set out with the sulphate of copper, the metal is deposited upon the platinode, and the compound *anion* (S + 4 O) travels to the acidulated water, but meeting with nothing with which it can combine, the decomposition of the water commences, the hydrogen of which combines with one equivalent of the oxygen of the compound anion (S + 4 O), and sulphuric acid (S + 3 O) remains; the current at the same time passes on with the equivalent oxygen of the water, which is either given off by the platinum zincode or absorbed by the zinc.

Another obvious point of great interest was, to ascertain what relations to the current the products of the electrolysis of the salts of ammonia would exhibit, and I proceeded as follows.

Experiment 16.—The double diaphragm cell, with a tin zincode, was charged with a strong solution of muriate of ammonia, and a voltameter was included in the circuit. The gas from the platinode was collected over mercury. The experiment was stopped when 35 cubic inches of mixed gases had been collected from the voltameter.

No gas was given off from the zincode; but the loss of the tin was 30·4 grains. 23·5 cubic inches of hydrogen were collected from the platinode, and the solution of that cell smelled very strongly of ammonia, and, upon neutralization, was found to contain 8¼ grains of the volatile alkali in a free state. The approximation of these numbers to equivalent proportions will be seen in the following Table:

	From Experiment. Cubic inches.	From Calculation. Cubic inches.
Mixed gases from voltameter	35·0	35·4
Hydrogen from platinode	23·5	23·6
Tin	30·4	29·0
Ammonia	8·25	8·5

Muriate of ammonia, therefore, proved to be an electrolyte whose simple *anion* was chlorine, and compound *cathion* nitrogen with four equivalents of hydrogen. Its electrolytic symbol therefore, instead of being (C^h + H) + (N 3 H), would be C^h + (N + 4 H).

Experiment 17.—The double cell was now charged with a strong solution of sulphate of ammonia, and the experiment conducted as before. Thirty-five cubic inches of mixed gases were collected from the voltameter, 11·5 cubic inches of oxygen from the zincode, and 23·0 cubic inches of hydrogen from the platinode. The zincode solution was acid, and the platinode by neutralization showed eight grains of free ammonia.

	By Experiment. Cubic inches.	By Calculation. Cubic inches.
Mixed oxygen and hydrogen	35	35·4
Oxygen from zincode	11·5	11·66
Hydrogen from platinode	23·0	23·32
Ammonia	8·0	8·5
Sulphuric acid		20·0

To explain these results, we must take into consideration that sulphate of ammonia is not, in a chemical point of view, a mere compound of sulphuric acid and ammonia, but that an equivalent of water is essential to its existence. Its formula is $(S + 3\,O) + (N + 3\,H) + (H + O)$; and by electrolysis it is resolved into one equivalent of sulphur with four equivalents of oxygen given off at the zincode, and one equivalent of nitrogen and four equivalents of hydrogen disengaged at the platinode. As an electrolyte its formula would therefore be $(S + 4\,O) + (N + 4\,H)$. Both the compound *anion* and the compound *cathion* agree perfectly in constitution with those which we have previously found evolved from their combinations with simple substances, at their respective anodes and cathodes.

It is impossible, I think, not to be struck with the singular, and, by me, perfectly unexpected coincidence of the results which I have just detailed with two celebrated hypotheses; the one of Berzelius, with regard to the constitution of the muriate of ammonia, and the other of Davy, concerning the nature of the aqueo-acids and their saline compounds.

The former has been led to imagine, from analogies which it will be quite unnecessary to recapitulate at present, that muriate of ammonia is a chloride of a hypothetical radical, which he has denominated *ammonium*, and which is constituted of one equivalent of nitrogen and of four equivalents of hydrogen; and the oxide of this radical he considers as the basis of the oxysalts of ammonia. According to this view, muriate of ammonia is represented by the formula $(N + 4\,H) + C^h$, and sulphate of ammonia by $(N + 4\,H + O) + (S + 3\,O)$. The former agrees exactly with the conclusion which we have derived from the electrolysis of the salt; but the latter differs from the electrolytic view, which is not that of a combined acid and base, but of *ammonium* and a compound anion $(S + 4\,O)$.

The hypothesis of Davy was, that the salts of the oxyacids might have an analogous constitution to that of the binary compounds of chlorine and metals, and that the aqueo-acids might be regarded as hydro-acids. As muriatic acid, therefore, is a compound of the simple radical chlorine and hydrogen, or $C^h + H$, aqueo-sulphuric acid may be a compound of a compound radical and hydrogen, or $(S + 4\,O) + H$.

When muriatic acid is brought to act upon soda, water is formed and chloride of sodium, or $C^h + Na$. When aqueo-sulphuric acid is made to act upon soda, water is also formed, and a binary combination of the compound radical and sodium, or $(S + 4\,O) + Na$.

The general view which he propounded was, that a *radical* (which might be either

simple or compound, as chlorine or cyanogen, or $(S + 4\,O)$ forms an acid with hydrogen, and a salt with sodium or any other metal. It was supported by many analogies; and it certainly has the advantage of assimilating in constitution a natural group of bodies which resemble one another so closely as the salts, and which former theories separated into the two dissimilar classes of *oxysalts* and *haloid* salts. The progress of organic chemistry, and the doctrine of substitutions, has lately added to the presumptive evidence which favours this hypothesis, and the results of electrolysis which I have just stated will probably be reckoned as direct evidence of its correctness. The only phenomena which do not fall within its comprehension, are those of the decomposition of the dilute sulphuric acid; as there appears no reason why the aqueo-acid should not be resolved into sulphuric acid, and one equivalent of oxygen at the zincode, and hydrogen at the platinode, or $(S + 4\,O) + H$ instead of $\left(\frac{S + 3\,O}{4} + O\right) + H$.

If we regard the water as the electrolyte which yields upon this occasion, it is no less difficult to understand the quarter equivalent of sulphuric acid which accompanies the oxygen to the zincode, and with which the facility of the convection is connected. It is not, however, wholly dependent upon it; for although, as we have seen, the quantity of sulphuric acid which passes is the same in all cases, the facility of electrolyzation decreases as the proportion of acid falls below one part in nine of the mixture.

The formation of these secondary electrolytes, and compound anions and cathions, will probably furnish the key to the explanation of many of those chemical compositions and decompositions to which the presence of water is necessary, such as those of nitric acid with the metals, and to the formation of SCHÖNBEIN's circuit; but the experimental examination of the hypotheses, and of the general question, must be left to a future opportunity. In the mean time,

I remain, my dear FARADAY,

Your faithful friend,

J. FREDERIC DANIELL.

King's College, London,
14*th May*, 1839.

POSTSCRIPT.

I have since ascertained that in aqueous solutions of the fixed alkalies the oxides travel in the contrary direction to that of the acid in the dilute sulphuric acid, and accumulate at the platinode. The amount transferred is in all cases below that of the equivalent of the gases evolved. I am at present engaged in fixing the exact relative proportions, and hope soon to communicate to you the results of my experiments.

J. F. D.

15*th June.*

VIII. *On a new Equiatomic Compound of Bicyanide with Binoxide of Mercury.* By James F. W. Johnston, *Esq. M.A. F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

Received March 21,—Read April 11, 1839.

When hydrocyanic acid of considerable strength (10 to 20 per cent.) is agitated with red oxide of mercury in large excess, a white compound is obtained, intermixed with the red oxide, on which cold water has very little action. If the mixture be collected on the filter and treated with boiling water, the new compound is separated from the excess of oxide, and, as the solution cools, is deposited on the sides of the vessel in the form of a white incrustation, adhering strongly, and consisting of an aggregation of colourless transparent four sided acicular prisms. In favourable circumstances, I have obtained it in such prisms half an inch in length, but not sufficiently perfect to admit of measurement.

This salt is remarkably distinguished from the bicyanide by its sparing solubility in cold water, by the strong alkaline reaction exhibited by its solution, and by its relations to heat. Heated gently in the air, it blackens slightly and then explodes with little noise, but if it be heated in larger quantity (5 to 10 grs.), and in a close tube, it explodes with a loud detonation, and shivers the tube into fragments. It does not explode under the blow of a hammer.

Burned with bichromate of potash or oxide of copper it gives off a gas which consists of nitrogen and carbonic acid, in the proportion of one volume to two. Thus

1.	131 vols. treated with caustic potash left	47
	239 vols. treated with caustic potash left	79
2.	122 vols. treated with caustic potash left	40
	91 vols. treated with caustic potash left	31·5
	590	197·5

and $197{\cdot}5 \times 3 = 592{\cdot}5$.

The formation of this new salt therefore is not due to any decomposition of the cyanogen.

Dissolved in dilute muriatic acid and gently heated, it emits the odour of prussic acid. If sulphuretted hydrogen be passed through the solution in water, a precipitate, at first white and afterwards becoming black, is thrown down. To the solution filtered and heated to drive off the excess of hydrosulphuric acid, sulphate of iron and afterwards caustic potash were added, when a precipitate of Prussian blue fell. The salt therefore contains

1. Cyanogen, since its solution when decomposed by hydrosulphuric and hydrochloric acid gives hydrocyanic acid.

2. Its alkaline reaction indicates an excess of mercury.

3. That this excess is in the state of oxide is shown by the detonating property of the salt, the sudden decomposition being due to the action of the oxygen on a portion of the carbon of the cyanogen.

Heated to incipient decomposition the salt loses no weight, it is therefore anhydrous.

29·275 grains dissolved in hydrochloric and precipitated by hydrosulphuric acid gave 29·245 grains of bisulphuret of mercury = 86·191 grains of metallic mercury per cent.

29·27 grains gave 29·15 grains bisulphuret = 85·933 Hg per cent.
34·93 grains gave 34·68 grains bisulphuret = 85·674 Hg per cent.

The last result, as we shall see, is nearest the truth.

Burned with oxide of copper,

63·55 grains gave $\ddot{C}$ = 11·96 = 5·203 carbon per cent.
$\dot{H}$ = 0·60

43·94 grains gave $\ddot{C}$ = 8·00 = 5·034 carbon per cent.

The small quantity of water obtained in the first analysis was obviously accidental, and therefore the water was neglected in the second. And since the nitrogen is to the carbon, as already shown, in the proportion of one volume or atom to two, 5·203 carbon, the result of the two very approximate ones, which should be nearest the truth, indicates 6·025 of nitrogen, since,

152·875 : 177·036 : : 5·203 : 6·025.

Therefore the salt consists by analysis of

Carbon	=	5·203
Nitrogen	=	6·025
Mercury	=	85·674
Oxygen	=	3·098
		100

This composition agrees with the formula ($Hg\ Cy_2 + Hg\ O_2$) which gives

			per cent. calculated.	per cent. by experiment.
2. Cyanogen	4 carbon	= 305·750 =	5·162	5·203
	2 nitrogen	= 354·072 =	5·977	6·025
	2 mercury	= 5063·288 =	85·483	85·674
	2 oxygen	= 200·000 =	3·378	3·098
		5923·110	100	100

This salt exhibits many very interesting reactions.

1. Heated over a lamp, under water, it becomes yellow, and a portion of a yellow powder (Hg Cy ?) ultimately remains undissolved, mixed with protoxide and a little metallic mercury; that is, a portion of it is decomposed, probably according to the following formula

$$(Hg\,Cy_2 + Hg\,O_2) + 3\,H\,O = Hg\,Cy + Hg\,O + (N\,H_3 + 2\,C\,O_2)$$

the reduction of the mercury being the consequence of a second reaction.

2. Caustic ammonia throws down from a cold solution of this salt, a copious and bulky white precipitate; from a hot solution, the precipitate is slightly yellowish. The supernatant liquid, on evaporation, gives pure bicyanide of mercury. The precipitate when dried at 212° Fahr. and heated in a close tube over a lamp, gives off ammonia, water, and metallic mercury. It is probably the same compound which is obtained by digesting red oxide of mercury in caustic ammonia, and which is *supposed* to be represented by one or other of the formulæ

$$(3\,\ddot{H}g + 4\,N\,H_3 + 8\,\dot{H}) \text{ or } (\ddot{H}g + 2\,\overline{Hg + 2\,N\,H_2} + 12\,\dot{H}).$$

3. A solution of sal ammoniac throws down a yellow precipitate, the supernatant liquid having a strong alkaline reaction. On heating, the precipitate is redissolved, ammonia is given off, and the alkaline reaction disappears. The *direct* action here is the change of the binoxide into bichloride and the liberation of ammonia, according to the formula

$$(Hg\,Cy_2 + Hg\,O_2) + 2\,\overline{N\,H_4\,Cl} = Hg\,Cy_2 + Hg\,Cl_2 + 2\,\overline{N\,H_4\,O}.$$

The white precipitate is a secondary result, due to the action of the free ammonia (as above mentioned), and which consequently disappears when the ammonia is driven off by heat. In this case, however, since bichloride is present, the white precipitate will not be the same as is produced by the action of ammonia on the new salt (2.), but may consist in part or in whole of the long-known *hydrargyrum precipitatum album*, the true composition of which ($Hg\,Cl_2 + \overline{Hg + 2\,N\,H_2}$) has been recently established by Dr. Kane. This reaction is represented by the formula

$$2\,Hg\,Cl_2 + 4\,N\,H_3 = (Hg\,Cl_2 + \overline{Hg + 2\,N\,H_2}) + 2\,\overline{N\,H_4\,Cl}.$$

4. The fixed alkaline chlorides render the solution of the new salt milky, and throw down a small quantity of a white precipitate, the liquid becoming more strongly alkaline. In this case also, the chloride gives up its chlorine to the mercury, and the free alkali causes the increased causticity of the solution. The milkiness I attribute to the presence in small quantity either of ammonia, or of some neutral salt (formiate ?) of mercury, the decomposition of which by the alkaline chloride causes the formation of an equivalent portion of calomel. It is known how readily formic acid and ammonia are produced at the expense of cyanogen, when water is present*, and it would appear that the action of heat upon this new double salt in water very frequently gives rise to these products. In treating with boiling water the mixture of

* The reaction is thus represented: $N\,C_2 + 4\,H\,O = (H\,C_2\,O_3 + N\,H_3)$.

oxide of mercury and this salt, (which is produced on digesting prussic acid with the latter in excess,) in order to obtain the salt in solution, copious bubbles of gas are evolved, the smell of ammonia becomes perceptible, and much metallic mercury is precipitated*. On concentrating the solutions also, decomposition takes place, with deposition of metallic mercury; and when the crystallized salt is dissolved in boiling water, a black sediment in greater or less quantity is generally observable. In the solution therefore when cold, the presence either of a trace of a formiate or of any ammoniacal salt would account for the slight deposit which occurs when chloride of potassium is added to it. When chloride of magnesium is employed, the milkiness is more speedy and more distinct, a light bulky hydrate of magnesia being thrown down, as is to be expected, when we consider that on parting with its chlorine the magnesium is oxidized, and set at liberty in a liquid which can retain very little of it in solution.

If the salt be added in successive quantities to a concentrated boiling solution of chloride of potassium, it is largely dissolved, and on cooling, the beautiful pearly scales of the compound of bicyanide of mercury with chloride of potassium fall down. The supernatant liquid contains bichloride of mercury and caustic potash.

A solution of iodide of potassium produces analogous compounds and reactions, as does also a solution of cyanide of potassium.

5. The solution of this new salt (oxycyanide), as has been already stated, has a strong alkaline reaction. If it be added to a very dilute solution of nitric acid till the solution ceases to redden litmus, it will be found to dissolve with great ease and in large quantity. The solution by careful evaporation gives long, delicate, transparent, colourless, quadrangular prisms, or hexagonal plates and pearly scales, and crystallizes to the last drop.

At 212° Fahr. these crystals lose nothing, and therefore contain no water. Over the lamp in the open air they decompose with a flash of white light, giving off metallic mercury, and leaving a yellow residue. In a close tube they are decomposed with a slight detonation, and with the emission of red fumes.

38·334 grains of the oxycyanide, cautiously treated in this way, gave beautiful crystals, weighing 40·53 grains, equal to an increase of 5·728 per cent. The new compound therefore consists of

		Calculated.	Found.
One equivalent of oxycyanide ($Hg\,Cy_2 + Hg\,O_2$) .	= 59·23	94·285	94·272
One half equivalent of nitric acid, $\frac{1}{2}\,N\,O_5$ . . .	= 3·38	5·715	5·728
	62·61	100	100

This salt is readily soluble in water. What is chiefly interesting in regard to its constitution and properties is, that the alkaline reaction of the oxycyanide should be fully

* This second reaction is due to the decomposition of the formiate of ammonia by the agency of the binoxide of mercury thus: $(H\,C_2\,O_3 + N\,H_3) + Hg\,O_2 = (N\,H_3 + C\,O_2) + C\,O_2 + Hg$.

destroyed by so small a quantity of nitric acid. This fact would seem to show, that the bicyanide of mercury, and consequently other compounds of the same class, do really possess something of the characters of the true acids, having the power, if not of completely neutralizing a metallic oxide, at least of so far modifying its basic character as materially to weaken its neutralizing action on the oxygen acid.

6. This is further illustrated by the action of acetic acid on the oxycyanide. A weak solution of this acid dissolves it in large quantity, and if carefully saturated, the solution gives, on evaporation, a white salt having the odour of acetic acid, which loses nothing at 220° FAHR., but when heated in a close tube, blackens and gives off cyanogen, metallic mercury, and a strong odour of acetic acid. In preparing this salt, it is difficult so perfectly to neutralize the acetic acid, that, during the evaporation*, any excess that may be present should not, as it becomes concentrated, decompose some of the bicyanide which the solution contains. On the following results, therefore, I do not place much reliance, though they serve to show by how small a quantity of this acid the oxycyanide may be perfectly neutralized.

		Increase.
49	grains of oxycyanide gave 49·94 of the new salt	= 1·918 per cent.
34·567	grains of oxycyanide gave 35·36 of the new salt	= 2·294 per cent.
54·00	grains of oxycyanide gave 54·92 of the new salt	= 1·704 per cent.

Taking the atom of acetic acid at 623, these results indicate the addition of *one-sixth* of an equivalent of this acid to each equivalent of the oxycyanide, or that the new compound is $(Hg\,Cy_2 + Hg\,O_2 + \frac{1}{6}\,\overline{A})$. Thus we have

			Calculated per cent.	Found per cent.
One equivalent of the oxycyanide		= 5923	= 98·23	98·296
One-sixth equivalent of acetic acid	= 643	= 107	= 1·77	1·704
	6	6030	100	100

Though this is not a solitary instance in which one of acetic acid combines with six of base, the subacetate of lead $(6\,\dot{P}b\,O + \overline{A})$ being an analogous salt, yet I would be understood as representing the constitution above given for this salt as open to future correction.

I have tried also several other organic acids, and have found that with the benzoic and citric acids, soluble and crystallizable salts may be obtained, and with tartaric acid, a compound which, after being crystallized, is decomposed by water into a soluble and an insoluble portion. Oxalic acid gives a white precipitate, and the filtered solution, on evaporation, two salts; one in white prisms, the other in yellowish crystals. I have not subjected any of these compounds to analysis.

7. When an acid solution of nitrate of silver is poured into a hot saturated solution of the oxycyanide, a shower of beautiful pure white prismatic crystals falls.

* When a solution of the bicyanide is heated with acetic acid the odour of prussic acid becomes distinctly perceptible.

These crystals dissolve readily in hot water and re-crystallize on cooling. Heated in a close vessel they give off water, become opake, melt, and finally explode, with the beautiful purple flame characteristic of burning cyanogen.

Heated on the Water Bath.

33·125 grains lost 2·63 grains = 7·939 per cent.
31·075 grains lost 2·46 grains = 7·916 per cent.

On standing in a moist atmosphere for a few days, the dry salt recovers the whole of its water of crystallization.

The 33·125 grains, after drying as above, were dissolved in water and precipitated by muriatic acid. The chloride of silver obtained weighed 10·48 grains, = 23·834 per cent., or 37·533 of nitrate of silver. This salt therefore is the compound discovered by Wöhler ($Hg\ Cy_2 + Ag\ O\ .\ N\ O_5 + 4\ H\ O$), and which is composed of

		Calculated per cent.	Found per cent.
Bicyanide of mercury, 1 equivalent	= 2991·46	53·709	54·540
Nitrate of silver, 1 equivalent .	= 2128·64	38·214	37·533
Water, 4 equivalents	= 449·92	8·077	7·927
	5570·02	100	100

Many of the other interesting reactions of the oxycyanide may be predicted from its constitution, and its use as a reagent will occasionally be valuable to the chemist, from its affording a solution in which binoxide of mercury exists as such, and possessed of alkaline properties.

Durham, February 1839.

IX. *On the Constitution of the Resins.* By James F. W. Johnston, *Esq. M.A. F.R.S., Professor of Chemistry and Mineralogy in the University of Durham.*

Received March 21,—Read April 11, 1839.

THE object of the investigation, of which the present paper forms a part, is

1. To determine the relative composition of the various resins which occur in nature. Possessing so many properties in common, this large family of natural productions ought also to present many analogies in constitution.

2. To ascertain how far they may be considered as derivatives from one common radical; and

3. Whether it is possible to represent them all by one or more general formulæ.

I. *Resin of Mastic.*

Mastic resin is said to be obtained from the *Pistacea lentiscus*, and to be produced chiefly in the island of Chios. It occurs in drops or tears, which are transparent, and of a pale yellow colour. It melts at 212°, and emits a peculiar and not unpleasant odour. Fused in a retort it gives off an acid liquid in small quantity. If the heat be raised to 300° Fahr. and upwards, the melted mass froths up, and water and acid vapours are evolved. At a higher temperature a pale yellow liquid distils over very slowly, at first of the consistence of oil, but increasing in thickness as the process proceeds, water and acid being also given off during the whole process. What remains in the retort is of a black colour, and nearly insoluble in alcohol.

Digested in cold alcohol, mastic resin is in great part dissolved; what remains is nearly pure white, elastic, capable of being drawn out into long fibres of a silky lustre, and is a compound with alcohol of a second resin (B), very sparingly soluble in alcohol, but more largely in an alcoholic solution of resin A. Hence the solution obtained contains a portion of the second resin, which may be in a great measure precipitated by large dilution with alcohol.

I. The solution of resin A thus obtained is of a pale yellow colour. Distilled and evaporated at 212° Fahr., it gives a pale yellow resin flowing freely at that temperature, but at higher temperatures frothing up and giving off alcohol.

Of a portion fused at 350° Fahr. till it ceased to emit bubbles of vapour,

8·67 grains gave $\ddot{C} = 24·685$, and $\dot{H} = 8·18$
10·73 grains gave $\ddot{C} = 30·335$, and $\dot{H} = 9·80$
10·294 grains gave $\ddot{C} = 29·16$, and $\dot{H} = 9·614$

These results give per cent.

	(1.)	(2.)	(3.)
Carbon	= 78·729	78·172	78·328
Hydrogen	= 10·483	10·337	10·377
Oxygen	= 10·788	11·491	11·295
	100	100	100

These agree nearly with the formula $C_{40}\,H_{32}\,O_4$, which gives

Carbon	= 79·275
Hydrogen	= 10·355
Oxygen	= 10·370
	100

and was adopted by Rose as representing the constitution of colophony. The carbon, however, is deficient, and the hydrogen, according to the more recent analyses of Trommsdorf, Liebig, and Laurent, is in larger quantity than in either the pinic or sylvic acid. I therefore dissolved in alcohol a second portion of the mastic of the shops, diluted the solution largely* with alcohol to separate the resin B, evaporated and heated to 260° Fahr. for eighteen hours, stirring it occasionally as long as bubbles of vapour were evolved in any quantity.

Of this resin 10·38 grains gave $\ddot{C}$ = 29·785, and $\dot{H}$ = 9·48, or

	(4.)
Carbon	= 79·343
Hydrogen	= 10·147
Oxygen	= 10·510
	100

which is almost identical with the result of Rose for the crystallized resin of copaiva balsam.

II. Heated again in the air to 350° Fahr. it had lost its fragrant odour, and emitted white vapours. In this state 8·718 grains gave $\ddot{C}$ = 25·16 and $\dot{H}$ = 8·02 grains. These are equivalent to

	(5.)	$C_{40}\,H_{30}\,O_4$ gives
Carbon	= 79·800	79·79
Hydrogen	= 10·221	9·77
Oxygen	= 9·979	10·44
	100·000	100·000

The carbon in this analysis is the exact theoretical quantity required by Liebig's formula for the sylvic and pinic acids, but the hydrogen is still considerably in excess.

* The solution from which the resin first analysed was obtained had not been diluted, as I was not then aware that by this means resin B would be more effectually separated. The resin A of analysis 1, 2, 3, might therefore contain a portion of resin B.

The resin thus analysed, however (5.), had undergone a change of constitution by being heated to 330°, for, when treated with alcohol, a large portion of it refused to dissolve, remaining behind as a soft reddish resin, on which the alcohol of the shops appeared to have little action in the cold.

After taking up all that was soluble, the alcoholic solution was evaporated, and the resin heated for two hours to 260° Fahr. It was transparent, reddish yellow, and very beautiful. On analysis 10·965 grains gave $\ddot{C} = 31{\cdot}925$ $\dot{H} = 10{\cdot}742$ grains, which are equal to

	(6.)	
Carbon	= 80·507	= 40 atoms
Hydrogen	= 10·885	= 33·11 atoms
Oxygen	= 8·608	= 3·27 atoms
	100	

The soluble part therefore of the changed resin was no longer pure mastic resin A (see *infrà* analysis, Nos. 26, 27, 28.).

III. A third portion of the mastic of the shops was boiled in water for several hours, by which it was rendered white, opake, and less fusible, probably from the loss of a portion of volatile oil existing in the resin of commerce. It was then digested in a large quantity of alcohol, in which it now also dissolved more slowly. The solution was evaporated, and the resin obtained again boiled in water for a length of time to drive off the whole of the alcohol.

1. A portion of this resin was then heated to about 240° Fahr., when it fused freely and ran smooth, and when cold had the same beautiful colour and appearance as that previously analysed.

10·956 grains gave $\ddot{C} = 31{\cdot}35$, and $\dot{H} = 10{\cdot}136$, or per cent.

	(7.)
Carbon	= 79·122
Hydrogen	= 10·279
Oxygen	= 10·599
	100

2. A second portion was heated only to 212° for 48 hours, when it had become transparent and of a pale yellow colour. At this temperature it softened, but did not enter into perfect fusion. It still showed a tendency to form minute bubbles of gaseous matter.

10·713 grains gave $\ddot{C} = 30{\cdot}71$, and $\dot{H} = 10{\cdot}02$ grains, or per cent.

	(8.)
Carbon	= 79·265
Hydrogen	= 10·392
Oxygen	= 10·343
	100

The resin employed in these last analyses (7. and 8.) was entirely dissolved when treated with alcohol, No. 7. leaving only a trace of insoluble matter. They may be considered therefore as representing very nearly the constitution of the soluble resin of mastic. The formula $C_{40}\ H_{31}\ O_4$ agrees very closely with these results, and with that of analysis No. 4, thus:

		Calculated.	(7.) Exper.	(8.) Exper.	(4.) Exper.
40 Carbon	= 3057·480	= 79·531	79·122	79·265	79·343
31 Hydrogen	= 386·867	= 10·063	10·279	10·392	10·147
4 Oxygen	= 400·000	= 10·406	10·599	10·343	10·510
	3844·347	100	100	100	100

If this be the true composition, it shows a close approximation to the pinic and sylvic acids, and yet a cause for the difference in the sensible properties exhibited by these different kinds of resin. If such differences should actually be found to exist in nature, the striking fact will at once suggest itself, that under the general expression $C_{40}\ H_{32-x}\ O_4$ we may have a great variety of resins; and a still greater variety, exhibiting also more sensible differences in their physical properties under the more general one of $C_{40}\ H_{32-x}\ O_{4\pm y}$.

IV. *Action of Heat.*—We have seen that when resin A is heated to 260° Fahr. or upwards, it is partly decomposed, and an insoluble portion remains when it is treated largely with common alcohol in the cold. This resin is in mass of a reddish colour, and is soluble in boiling alcohol, from which it falls as a yellow powder on cooling. Dried at 212° Fahr. it does not melt, but at about 130° it begins to cohere into a dark reddish brown mass. Heated for twelve hours at 212°, and afterward exposed to a temperature at which it began to cohere,

A. 8·538 grains gave $\ddot{C} = 24·33$ and $\dot{H} = 7·776$

B. 10·994 grains gave $\ddot{C} = 31·245$ and $\dot{H} = 9·976$

These are equivalent to

	A. (9.)	B. (10.)
Carbon	= 78·762	= 78·943
Hydrogen	= 10·119	= 10·128
Oxygen	= 11·119	= 10·929
	100	100

2. As these results do not give any clear idea of what takes place when the resin is thus heated, I introduced a portion of the pure resin into a glass retort and kept it fused in a sand bath for twenty-four hours, the heat during the last twelve hours being raised to 270° Fahr. Water was deposited in the neck of the retort, rendered sour by a crystallizable acid in small quantity. The heat was withdrawn, when a

pale yellow liquid began to show itself in the upper part of the retort. The resin was red and exceedingly beautiful, and when treated with alcohol was now separable into three portions.

A. Boiled with alcohol of the shops, a portion of a red colour was left, on which this liquid appeared to have little further action. It adhered strongly to the retort, but was separated by boiling water, and obtained in the form of a dirty yellow powder, which, at 270° Fahr., showed no symptoms of fusion. Dried at this temperature

8·475 grains gave $\ddot{C} = 23·625$ and $\dot{H} = 9·859$, or

	(11.)	$C_{40} H_{30} O_5$ gives	$C_{40} H_{31} O_5$ gives
Carbon =	77·080	77·76	77·51
Hydrogen =	9·859	9·52	9·80
Oxygen =	13·061	12·72	12·69
	100	100	100

From a single analysis, which was all that the quantity at my disposal enabled me to perform, it is impossible to determine which of the formulæ represented in columns 2 and 3 is the true one. Probability, however, is in favour of the first, or $C_{40} H_{30} O_5$, which indicates that when decomposed by a moderate heat one of the changes which mastic resin A. undergoes is the substitution in a part of it, of an atom of oxygen for one of hydrogen, the new compound being nearly insoluble in common alcohol even at a boiling temperature.

B. The boiling alcoholic solution deposited on cooling a pale yellow resin, which heated and fused, became brownish red.

11·817 grains gave $\ddot{C} = 33·858$, and $\dot{H} = 10·356$ grains.

C. The cold alcoholic solution, when evaporated and heated gently till perfectly transparent, gave a beautiful reddish yellow resin, of which

12·05 grains gave $\ddot{C} = 34·003$, and $\dot{H} = 11·227$ grains.

These results are equal to

	B. (12.)	C. (13.)
Carbon =	79·225 =	78·026
Hydrogen =	9·737 =	10·352
Oxygen =	11·038 =	11·622
	100	100

From these analyses we cannot derive much information with regard to the constitution of these soluble portions of the changed resin, and it would be necessary to carry the process further than was done in this experiment, before the action of heat can be accurately understood. If we might be guided in our opinions as to the ultimate nature of the soluble part by the constitution of the small quantity of which

No. 6. exhibits the analysis, we should infer that two new resins are produced, in the formulæ for which are found O_5 and O_3 respectively. But the subject requires further investigation, and I hope to be able to return to it at a future period.

V. *Atomic weight of Resin A.* It has been already observed by UNVERDORBEN that the resin of mastic bears a considerable resemblance to colophony, and combines with nearly the same proportion of oxide of lead.

A. *Salts of Lead.* When a solution of this resin in alcohol is mixed with an alcoholic solution of acetate of lead, a white precipitate falls, which, when dried at 212° becomes yellow; at 350° FAHR. it melts into a brownish yellow mass without decomposition.

1. Dried at 300° FAHR. 11·610 grains left 2·307 grains of oxide of lead = 19·878 per cent.

2. Of the same *fused* at 350° FAHR., 10·33 grains left 2·07 grains of oxide = 20·038 per cent.

These give for the constitution of the salt

	1.	2.	
Resin . . .	80·122	79·962	. . . . (14.)
Oxide of lead .	19·878	20·038	
	100	100	

It is therefore a *sesquisalt* = $1\frac{1}{2}$ (C_{40} H_{31} O_4) + Pb O, which consists of

$$\left.\begin{array}{l}\text{Resin} = 80{\cdot}527\\ \text{Oxide} = 19{\cdot}473\end{array}\right\} = 100$$

When boiled in alcohol, after being dried at 300° FAHR., this salt is not wholly dissolved. Of that which was left undissolved in one experiment,

$$\left.\begin{array}{r}12{\cdot}17 \text{ grains left } 3{\cdot}09 \text{ oxide of lead} = 25{\cdot}31 \text{ per cent.}\\ 8{\cdot}805 \text{ grains left } 2{\cdot}20 \text{ oxide of lead} = 24{\cdot}99 \text{ per cent.}\\ \text{in another experiment}\\ 8{\cdot}57 \text{ grains left } 2{\cdot}232 \text{ oxide of lead} = 26{\cdot}04 \text{ per cent.}\\ 11{\cdot}491 \text{ grains left } 3{\cdot}026 \text{ oxide of lead} = 26{\cdot}33 \text{ per cent.}\end{array}\right\} \quad . \; . \quad (15.)$$

These results agree with the formula (C_{40} H_{31} O_4) + Pb O, which gives

$$\left.\begin{array}{l}\text{Resin} = 73{\cdot}388\\ \text{Oxide} = 26{\cdot}612\end{array}\right\} = 100$$

The boiling alcoholic solution deposits on cooling a white precipitate. After being dried at 300°, the portions of this precipitate obtained from the above two experiments were burned in the air.

$$\left.\begin{array}{r}\text{A. } 5{\cdot}21 \text{ grains left } 0{\cdot}994 \text{ oxide of lead} = 19{\cdot}079 \text{ per cent.}\\ 5{\cdot}52 \text{ grains left } 1{\cdot}044 \text{ oxide of lead} = 18{\cdot}913 \text{ per cent.}\\ \text{B. } 9{\cdot}217 \text{ grains left } 1{\cdot}683 \text{ oxide of lead} = 18{\cdot}249 \text{ per cent.}\end{array}\right\} \quad . \; . \quad (16.)$$

The salt thus precipitated is therefore the same as that obtained by the direct ac-

tion of the solutions of resin A. and of acetate of lead on each other. When boiled on this sesquisalt, therefore, the alcohol decomposes it, dissolving a *bi*salt and leaving a *neutral* salt insoluble, while from the solution of the former a *sesqui*salt precipitates on cooling. This accounts for a slight deficiency in the oxide of lead found in these salts; the presence of a small quantity of a more acid salt in each causing a higher percentage of resin than is given by calculation.

When to the acid solution containing acetate of lead and resin A. from which this *sesqui*salt has been separated by filtration, pure ammonia is cautiously added with repeated agitation; a further white precipitate falls, which is *probably* the neutral salt above described. I did not dry this salt, but transferred it whilst still moist into boiling alcohol. After repeated digestion a comparatively small portion only was dissolved, the boiling solution depositing the *sesqui*salt as it cooled. The insoluble salt was collected and dried at 300° FAHR.

5·93 grains left 2·452 of oxide of lead = 41·357 per cent.

It is therefore a *di*salt = $(C_{40} H_{31} O_4) + 2 Pb O$, which consists of

	By experiment.	Calculated.	
Resin =	58·643	57·96	. . . . (17.)
Oxide =	41·357	42·04	
	100	100	

It appears, therefore, that resin A. of mastic forms with oxide of lead at least four compounds.

a. $2 (C_{40} H_{31} O_4) + Pb O$ obtained in solution on boiling *b* in alcohol.

b. $3 (C_{40} H_{31} O_4) + 2 Pb O$ by mixing the alcoholic solutions of the resin and of acetate of lead, and by subsidence from boiling solutions of *a*.

c. $(C_{40} H_{31} O_4) + Pb O$ left insoluble when *b* is boiled in alcohol.

d. $(C_{40} H_{31} O_4) + 2 Pb O$ left insoluble when the fresh precipitate from ammoniacal solutions of the resin and of acetate of lead is boiled in alcohol.

The compound *a*, as will appear from the preceding detail, is hypothetical, but its existence is necessary to explain the action of alcohol on the *sesqui*salt, and is rendered almost certain by the existence of an analogous silver salt*.

B. *Salts of Silver.* When an *ammoniacal* solution of nitrate of silver in alcohol is poured into an alcoholic solution of mastic resin A, a pure white precipitate falls, soluble in large excess of ammonia, and becoming reddish brown in the sun's rays. Collected on the filter, and afterwards boiled with alcohol, it is nearly all dissolved, giving a slightly coloured solution, from which on cooling, a bulky white precipitate

* If from the solution of the *bi*salt a *sesqui*salt precipitate spontaneously, a more acid salt still must remain in solution. Three equivalents of a *bi*salt would allow two of a *sesqui*salt to fall, while one of a *ter*salt would remain in solution. I shall have occasion hereafter to mention other circumstances which render probable the existence of resinous salts which contain three equivalents of acid, and others which contain three of base.

again falls. This bulky precipitate was dried by pressure between folds of bibulous paper, and afterwards at a temperature of 250° FAHR.

Dried at 250°, and still in the state of a white powder, it left 15·788 per cent. of metallic silver when burned in the air.

At 300°, when it cohered, became of a dark colour, and gave a brownish red powder; it left 16·042 per cent. of silver. At 350° FAHR. when it began to fuse it left 15·844 per cent.

The mean of these three results is 15·891 per cent. of silver, or 17·066 of oxide of silver, therefore the salt consists of

$$\left.\begin{array}{l}\text{Resin} \quad . \quad . \quad . = 82{\cdot}934 \\ \text{Oxide of silver} = 17{\cdot}066\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$
$$\overline{100}$$

A compound represented by the formula $2\,(C_{40}\,H_{31}\,O_4) + Ag\,O$ should contain 15·88 per cent. of oxide of silver, from which it would appear that the above is a *bi*salt with a small excess of silver. That there are other salts of silver containing less acid is rendered probable by the difficulty of obtaining this *bi*salt without excess of base.

VI. *Constitution of the Resin Salts.*—There remains still one important question to be solved in regard to the constitution of the salts of this and the other resins. Does the resin unite with the oxide as a whole and without change, as muriatic acid unites with ammonia to form sal-ammoniac, or is it altered in any way, as the organic acids generally are when they combine with bases? Does it part with any of the oxygen or hydrogen it contains, when it forms a metallic salt? This inquiry is a very interesting one, and it is not at all answered by the experiments above detailed. The results obtained are sufficiently wide of the quantities indicated by theory, to admit of considerable changes in the constitution of the resin, the existence of which can only be discovered by the ultimate analysis of the several compounds. In the paper of ROSE on the constitution of colophony and some of the other resins*, the relative quantities of acids and base in several of the salts is investigated, but no ultimate analysis of them is given. He only says, that, by the ultimate analysis of the salts, he obtained nearly the same composition for the resin as by analysing the pure resin itself. From this it is fair to infer, that he found a slight difference, which he would naturally attribute to some impurity or imperfection in the salt employed, and would not, therefore, consider these results to be deserving of equal confidence with those obtained from the pure resin. In the actual state of our knowledge in regard to the theory of the saline compounds of organic substances possessing acid properties, it would be a great step in advance were the constitution of the resinous salts accurately made out, and it might be expected to throw much light on the relative constitution of the different resins themselves.

* POGGENDORFF's Annalen, xxxiii. p. 32.

1. Of the neutral salt of lead above described, 11·9 grains gave $\ddot{C} = 25·14$, and $\dot{H} = 7·372$ grains. This is equal to

	(19.)	$PbO + (C_{40} H_{31} O_4)$ gives
Carbon . . =	58·416	58·361
Hydrogen . =	7·372	7·384
Oxygen . . =	7·882	7·643
Oxide of lead =	26·333	26·612
	100	100

2. Of the sesquisalt of lead $2\,PbO + 3\,(C_{40} H_{31} O_4)$ 11·28 grains gave $\ddot{C} = 26·732$, and $\dot{H} = 8·446$ grains. This is equal to

	Experiment.	Calculated.	
Carbon . . =	65·529	64·044	. . . (20.)
Hydrogen . =	8·446	8·103	
Oxygen . . =	7·776	8·379	
Oxide of lead =	18·249	19·477	
	100	100	

The first of these results is very near the theoretical constitution, and could it be *exclusively* depended upon, would indicate $C_{40} H_{30} O_4$ as the formula for the resin. The second analysis exhibits an excess of carbon, but the salt was not pure, as the quantity of lead it contained indicates. Want of material prevented me from repeating the analysis. Both results, however, are equally satisfactory in showing, that, in combining with the oxide of lead, the resin parts with none of its oxygen, and that the base does not replace any of the elements which it contains in an uncombined state.

3. Of the *sub*salt $2\,PbO + (C_{40} H_{31} O_4)$ dried at 300° Fahr.

A. 13·6 grains gave $\ddot{C} = 22·54$, and $\dot{H} = 7·064$

B. 14·11 grains gave $\ddot{C} = 24·00$, and $\dot{H} = 7·127$

C. 8·925 grains gave $\ddot{C} = 14·77$, and $\dot{H} = 4·595$.

These are equal to

	A. (21.)	B. (22.)	C. (23.)
Carbon . . =	45·827	47·033	45·760
Hydrogen . =	5·767	5·612	5·720
Oxide of lead =	41·355	41·355	41·355
Oxygen . . =	7·049	5·998	7·165
	100	100	100

The formula requires

40 Carbon . . =	3057·48 =	46·09
31 Hydrogen . =	386·86 =	5·86
2 Oxide of lead =	2788·99 =	42·04
4 Oxygen . . =	400·00 =	6·01
	6633·34	100

The second analysis B. exhibits a considerable excess of carbon, probably from an error of analysis. All the three, however, agree in representing the hydrogen as less than the formula adopted for the resin requires. A salt consisting of $(C_{40} H_{30} O_4) + 2$ Pb O would contain

Carbon . .	=	46·16
Hydrogen .	=	5·65
Oxide of lead	=	42·12
Oxygen . .	=	6·07
		100

The quantity of hydrogen is even sufficiently small to allow us to represent the resin by $(C_{40} H_{29} O_4)$, while the neutral salt appears to contain $(C_{40} H_{30} O_4)$. We can draw no inference in regard to the amount of hydrogen it contains from the analysis of the sesquisalt, from its apparent impurity; but in the other salts of lead the hydrogen present is manifestly less than in the pure resin according to the analyses above given (4, 7 and 8.). Can the metal replace the hydrogen, giving in the one $C_{40} H_{30} Pb_1 O_5$, and in the other $C_{40} H_{29} Pb_2 O_6$, and can such be the general constitution of the resin salts? This can only be determined by numerous and refined analyses of these compounds.

4. The salt of silver prepared, as already described, by precipitation and subsequent resolution in alcohol, and containing 16·954 per cent. of oxide of silver, gives a different result from those of lead in regard to the constitution of the resin.

A. 12·565 grains after heating to 300° Fahr., when it began to fuse, gave $\ddot{C} = 30·74$, and $\dot{H} = 9·704$.

B. Heated to 250° only, 11·38 grains gave $\dot{C} = 28·293$, and $\dot{H} = 8·9$ grains.

These results give per cent.

		A. (24.)	B. (25.)	Calculated.
Carbon . . .	=	67·595	68·673	68·589
Hydrogen . .	=	8·581	8·689	8·398
Oxygen . . .	=	6·870	5·684	6·732
Oxide of silver	=	16·954	16·954	16·281
		100	100	100

The third column is calculated according to the formula $2 (C_{40} H_{30} O_3) + Ag O$, and approximates as closely to the experimental results as was to be expected from a salt containing an excess of oxide, probably from the presence in small quantity of a less acid compound.

It appears, therefore, that in uniting with oxide of silver, the elements of an atom of water have been given off, the resin $C_{40} H_{31} O_4$ becoming $C_{40} H_{30} O_3 + H O$. And yet that this is not owing to a simple replacement of hydrogen by silver in the salt, is shown by its containing two equivalents of resin to one of oxide, 2 H O being given off while Ag O only is taken up. It is by no means clearly deducible from these

analyses, therefore, that the constitution of the resin of mastic A. is truly represented by $C_{40}\ H_{30}\ O_3 + H\ O$, since during the treatment with silver and ammonia some deeper change may have been effected, giving rise to a new resin represented by $C_{40}\ H_{30}\ O_3$.

And that this is really the case, seems to be confirmed by an examination of the ammoniacal alcoholic solution from which the salt of silver has been precipitated. When this solution is decanted, and by distillation is concentrated and deprived of its excess of ammonia, it becomes of a brown colour, and a dark brown resin falls to the bottom of the retort or of a jar, into which the solution may be poured. On further concentration, after cooling, more of this resin falls, after which a precipitate of a white silver salt makes its appearance.

Separated by decantation, and kept for twelve hours at 212° Fahr., at which temperature it melted readily,

A. 5·089 grains of this brown resin burned in the open air, left 0·85 of silver, equal to 1·786 per cent. of oxide.

B. 10·97 grains burned with oxide of copper, gave $\ddot{C} = 31·67$, and $\dot{H} = 10·565$.

Correcting B by A, the resin consists of

	(26.)	$C_{40}\ H_{32}\ O_2$ gives
Carbon	= 81·280	81·385
Hydrogen	= 10·895	10·620
Oxygen	= 7·852	7·995
	100	100

When dissolved in alcohol a small quantity of a dark brown silver salt was separated. After filtration, evaporation, and fusing at 212° for several hours, the resin was obtained nearly free from silver.

A. 3·061 grains left, when burned in the air, 0·016 = 0·522 per cent.

B. 9·158 grains gave $\ddot{C} = 26·525$, and $\dot{H} = 9·0$.

C. After heating for several hours at 300° Fahr., during which a fragrant odour was emitted, 10·155 grains gave $\ddot{C} = 29·40$, and $\dot{H} = 9·933$.

These gave for the constitution of the resin

	A (27.)	B (28.)
Carbon	= 80·510	80·474
Hydrogen	= 10·976	10·925
Oxygen	= 8·514	8·601
	100	100

If we admit, as is very probable, that the excess of hydrogen and oxygen indicated by these analyses over those obtained in analysis 26. is due to the presence of a small quantity of water, even after heating at 300° Fahr., the three results become entirely accordant*, and leave little doubt that the constitution of the resin is ex-

* The discordance in the results thus obtained for the same mass, of the same resin before and after solution

pressed at least very approximately by $C_{40} H_{32} O_3$. In looking back upon our previous analyses also, we find in No. 6. a very close agreement with these now given; from which, compared with No. 11., it would appear that by the long continued action of a temperature not below 350° FAHR. the resin of mastic A, besides other alterations, is changed into a soluble resin containing *three*, and into an insoluble resin containing probably *five* equivalents of oxygen.

The constitution of the resin formed during the preparation of the silver salt, seems thus to confirm the opinion above stated, that a deeper change is produced upon resin A during the treatment with nitrate of silver and ammonia, than the simple extrication of the elements of an atom of water. The large quantity of hydrogen present in the new resin especially supports this view. In the silver salt we have taken $C_{40} H_{30} O_3$ as the most probable expression for the resin it contains, assuming, as is usually done, that the lowest amount of hydrogen obtained by analysis is greater than the truth. In the analysis of the resin itself the same precautions were taken, yet $C_{40} H_{32} O_3$ contains the lowest amount of hydrogen which the results permit us to adopt. The presence of moisture in the oxide of copper could not, with the precautions adopted, occasion so great an excess of hydrogen; and in analysis (26.) there is no corresponding excess of oxygen to justify us in attributing the difference to hygrometric water in the resin itself.

5. With the view of investigating more closely the action of oxide of silver on the resin A, I dissolved a portion of it in alcohol, added to the solution a little caustic ammonia, and afterwards an alcoholic solution of nitrate of silver. The white salt which fell was collected on the filter, washed with cold alcohol, and afterwards dried at 300°. It melts at a low temperature in the alcohol it retains, and is obtained in the form of a dark red porous mass. By a careful regulation of the heat, however, it may be dried without undergoing fusion.

5·177 grains, burned in the air, gave 0·923 of metallic silver = 17·829 per cent.

4·404 grains gave 0·79 = 17·94 per cent. of silver, or 19·26 of oxide of silver.

11·632 grains gave with oxide of copper $\ddot{C}$ = 26·87, and $\dot{H}$ = 8·69.

These results give for the composition of the salt

	(29.)	In equivalents.
Carbon	= 63·87	= 40
Hydrogen	= 8·30	= 31·8
Oxygen	= 8·57	= 4·1
Oxide of silver	= 19·26	= 0·6
	100	

in alcohol, is not without interest in a research into the general constitution of the resins. It shows that, with all our precautions, small differences will occur from circumstances not yet understood, and that in our selection of a formula to represent the constitution of a given resin, we ought to allow *analogy* to influence us in some degree where the results of analysis are not sufficiently numerous and concordant unequivocally to settle the question. To this point I shall have occasion to revert in a future paper on the constitution of certain other resins.

The resin in this salt is evidently $C_{40}\,H_{31}\,O_4$, or in combining with the silver by this method of preparation it undergoes no change. There is present a considerable excess (one-sixth of the whole) of silver, probably from an admixture of a less acid salt.

After the separation of the salt thrown down as above, nitrate of silver was still added as long as a precipitate fell. This second precipitate was collected and digested in boiling alcohol. A transparent slightly brownish solution was obtained, from which on cooling a white compound fell, but at the same time a considerable portion of a dark brown resin or resinous salt was precipitated on the sides and bottom of the retort in which the solution was effected. The white precipitate was collected and dried at 300° Fahr.

4·167 grains left 0·64 of metallic silver = 15·36 per cent., or 16·496 of oxide of silver.

A. 11·975 grains, burned with oxide of copper, gave $\dot{C}$ = 28·62, and $\dot{H}$ = 9·195.

B. 8·67 grains gave $\dot{H}$ = 6·703, the carbonic acid being lost.

The salt therefore consists of

	A. (30.)	B.	$2\,(C_{40}\,H_{31}\,O_4) + Ag\,O$ gives
Carbon	= 66·086		66·79
Hydrogen	= 8·530	8·59	8·46
Oxide of silver	= 16·496		15·88
Oxygen	= 8·888		8·87
	100		100

The bisalt of oxide of silver therefore, prepared without large excess of ammonia, is not decomposed by solution in pure alcohol, the change produced upon it and upon the resin in the former experiment being due either to the excess of ammonia or to the long boiling. Still though the salt obtained in this instance contained the unchanged resin A, yet when the alcoholic solution from which the precipitates were originally thrown down, was concentrated by distillation, there fell, on allowing the solution to cool, first, a portion of a white precipitate, and afterwards, on still further concentration, a quantity of a dark coloured resin, closely resembling the $C_{40}\,H_{32}\,O_3$, above described and analysed. It appears therefore to be easily reproducible, though the circumstances *necessary* to its production require further investigation.

In regard to the hydrogen contained in this salt, the analysis seems to indicate thirty-one equivalents, which does not agree with the change undergone by the resin in combining with the oxide of lead. It is not impossible, however, that the true formula for the salt may be $Ag\,O + (C_{40}\,H_{30}\,O_4) + (C_{40}\,H_{31}\,O_4)$, which would give 8·34 per cent. of hydrogen, a quantity not differing from 8·53 found by analysis, more than the ordinary results of experiment do from the calculated results; but upon this subject I would not be understood to express a decided opinion until it has undergone further examination*.

* In all these analyses the method of Liebig has been adopted, with the use of the cork; there seems, however, much weight in the observation of Berzelius, that where the number of atoms of hydrogen is great this method may lead to error. While a damp cork may give out, a perfectly dry one may absorb moisture.

VII. *Insoluble Resin of Mastic* (Resin B.). When crude mastic resin is digested in cold alcohol a variable quantity remains undissolved. This portion is soft, tenacious, and elastic like bird-lime, of a silky lustre, and may be boiled in alcohol without sensible diminution. When boiled in water it becomes harder and less elastic; but when deprived of water by drying at 300° FAHR., at which temperature it is in a semifused state, it may be drawn out into long fibres, and even when cold it is tough and has some of the elasticity of caoutchouc.

1. Burned in the open air it left a small gray residue of carbonate of lime, being earthy matter mixed with the natural resin.

6·358 grains left 0·06 = 0·945 per cent.

2. Burned with oxide of copper after drying at 300° FAHR.

A. 12·863 grains gave $\ddot{C}$ = 38·624, and $\dot{H}$ = 12·645.

B. 11·605 grains gave $\ddot{C}$ = 34·75, and $\dot{H}$ = 11·395.

These results, allowing for the small quantity of foreign matter present (0·954 per cent.), are equivalent to

		(31.)		(32.)
Carbon	=	83·823	=	83·589
Hydrogen	=	11·027	=	11·013
Oxygen	=	5·160	=	5·398
		100		100

The formula which approaches nearest to these results is $C_{40}\,H_{31}\,O_2$, giving

40 Carbon	=	3057·480	=	83·613
31 Hydrogen	=	399·347	=	10·920
2 Oxygen	=	200·000	=	5·467
		3656·827		100

The slight excess of carbon in the first analysis is probably due to some accidental cause. It may have been from a greater admixture of foreign matter in the one portion than in the other, that the small difference has arisen.

When this resin is exposed to a higher temperature it swells, gives off the odour of naphtha (?), and the porous mass, when cold, is no longer soft but brittle. Analysed in this state it gave the formula ($C_{40}\,H_{31}\,O_3$), but I did not ascertain how much foreign matter this specimen contained, and the coincidence of the result with this formula may be accidental.

This resin does not sensibly dissolve in a solution of caustic potash, it is, therefore, most probably not an acid resin.

VIII. *Conclusions.*—In regard to the resin of mastic, it therefore appears, from the preceding examination,

1. That it consists of two resins represented respectively by the formulæ (analyses 4, 7, 8, and 31, 32).

The soluble, or resin A. by $C_{40}\ H_{31}\ O_4$ an acid resin.

The insoluble, or resin B. by $C_{40}\ H_{31}\ O_2$ not an acid.

2. That even when considerable care is taken, a series of analyses may be obtained which do not indicate the true constitution of a resin (see analyses 1, 2, 3.).

3. That when exposed to the prolonged action of a temperature exceeding 300° FAHR., resin A. of mastic, among other changes, seems to be partly altered into a resin containing three, and into one containing five of oxygen, C_{40} being constant (analyses 6, and 11.). These resins are probably represented by

$A = C_{40}\ H_{32}\ O_3$, very soluble in alcohol.
$B = C_{40}\ H_{30}\ O_5$, sparingly soluble in alcohol.

4. That it combines with bases forming four series of salts, which in the case of oxide of lead consist of (see analyses 14, 15, 16, 17.),

1. Two equivalents of resin and one of oxide.
2. Three equivalents of resin and two of oxide.
3. One equivalent of resin and one of oxide.
4. One equivalent of resin and two of oxide.*

5. That in combining with bases, resin A. does not part with any of its oxygen, but that *if any change take place* in its constitution it is in the hydrogen being replaced by an equivalent proportion of a metal (analyses 19, 21, 22, 23, 24, 25, and 30.). The salts of lead would on this view be represented by

1. $C_{40}\ H_{31}\ O_4 + C_{40}\ H_{30}\ Pb_1\ O_5$.
2. $C_{40}\ H_{31}\ O_4 + 2\ (C_{40}\ H_{30}\ Pb_1\ O_5)$.
3. $C_{40}\ H_{30}\ Pb_1\ O_5$.
4. $C_{40}\ H_{29}\ Pb_2\ O_6$.

I regret to be obliged to leave this interesting part of the subject in an undetermined state, but it will require many often repeated analyses to determine whether this law be generally obeyed by the resins in their combinations with bases.

6. That resin A. by boiling in contact with ammonia and nitrate of silver, or perhaps with nitrate of ammonia†, is changed into a resin, represented, when uncombined, by $C_{40}\ H_{32}\ O_3$. This resin forms a *bi*salt with oxide of silver, in which there

* A notation for the resins is very desirable. If the initial letter of the name be adopted, and *r* be put over it to denote resin, and *a*, *b*, be subjoined to distinguish the several resins contained in one natural product, a notation would be obtained easily intelligible, occupying little space, and which would interfere with no other chemical notation yet adopted. Thus the two resins of mastic would be represented by $\overset{ra}{M}$, $\overset{rb}{M}$, and the salts of lead formed by resin A. by $\dot{P}b\ \overset{ra}{M}_5$, $2\ \dot{P}b + 3\ \overset{ra}{M}$, $\dot{P}b\ \overset{ra}{M}$, $2\ \dot{P}b + \overset{ra}{M}$.

† Since the above was transmitted to the Royal Society I have tried the effect of boiling resin A. with nitrate of ammonia. Solutions of this salt and of resin A. were mixed and repeatedly distilled, with fresh additions of alcohol. The resin became darker in colour, and much more soluble in boiling than in cold alcohol. After

is also an apparent replacement of hydrogen by silver*, the resin in this salt (analyses 24 and 25) being represented by $C_{40}\ H_{30}\ O_3$.

II. *Resin of Dragon's Blood.*

Two varieties of dragon's blood are common in this country; one in long stalks or quills inclosed in a leaf, or reed, and presenting the appearance of having been softened by heat previous to being rolled into this form; the other in lumps of considerable size, less compact, and mixed with a large quantity of vegetable and other foreign matter.

1. The purer variety in quills gave, on digestion in alcohol, and evaporation of the solution on the water bath, a resin almost black by reflected light, translucent, in thin fragments, and of a bright red by transmitted light, and in powder of a dark red colour. In preparing these resins it is difficult to determine at what temperature the whole of the alcohol or ether is driven off, and when the frothing up is due to the decomposition or disengagement of volatile matter from the resin itself. This is especially the case in regard to such as, like the present, melt at a low temperature. If the solution in alcohol or ether be evaporated at 180° Fahr. till it flows smooth and ceases to change in consistency, it will be found still to froth up and give off vapours having a peculiar, at first agreeable, afterwards *astringent* (?)† odour, if the heat be raised to 212°. That a change of composition ensues from a greater increase of temperature appears from the following analyses.

A. A portion of the solution in alcohol was evaporated and heated for a short time at 212° Fahr. 11·3 grains gave $\ddot{C} = 29{\cdot}43$, and $\dot{H} = 6{\cdot}084$ grains.

B. A second portion was evaporated at 212°, and afterwards raised for a few minutes to 220° Fahr. 11·16 grains gave $\ddot{C} = 29{\cdot}04$, and $\dot{H} = 6{\cdot}135$ grains.

C. The same heated to 280° Fahr. frothed up, emitting vapours having an *astringent* alcoholic odour, accompanied with considerable pungency. It was still entirely soluble in alcohol.

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boiling in water and drying at 300° Fahr., 10·04 grains gave $\ddot{C} = 28{\cdot}89$, and $\dot{H} = 9{\cdot}445$ grains, or per cent.

$$\left.\begin{array}{ll}\text{Carbon} & = 79{\cdot}56\\ \text{Hydrogen} & = 10{\cdot}45\\ \text{Oxygen} & = \ \ 9{\cdot}98\end{array}\right\}100.$$

By comparing this result with analyses 4, 7, and 8, it will be seen that the resin had undergone no change in composition, but was still $C_{40}\ H_{31}\ O_4$.

* If the resin be $C_{40}\ H_{31}\ O_3$, then $(C_{40}\ H_{31}\ O_3) + (C_{40}\ H_{30}\ O_3) + Ag\ O$ would agree satisfactorily enough with analyses 24 and 25.

† I venture to use this term, as it, more nearly than any correct one, describes the sensation.

12·42 grains gave $\ddot{C}$ = 32·965, and $\dot{H}$ = 6·94 grains.
These results are equivalent to

	At 212°.	At 220°.	At 280°.
Carbon	= 72·015	71·887	73·435
Hydrogen	= 5·982	6·108	6·208
Oxygen	= 22·003	22·005	20·357
	100	100	100

Had the results contained in the first and second columns been obtained from the same specimen, we might have concluded that up to 220° Fahr. no sensible decomposition takes place. As however these results, though they approximate to the formula C_{40} H_{20} O_9, are yet not *accurately* represented by any formula in which the carbon is expressed by C_{40}, I thought it necessary to analyse the resin prepared by evaporating a solution at a temperature never exceeding 180° Fahr.

2. For this purpose I took the common dragon's blood in lumps, and digested two separate portions in alcohol and ether, and filtered and evaporated the solutions in broad shallow dishes. The resin thus obtained was kept for twelve hours in a stove, the temperature of which was generally considerably under 180° Fahr. At this temperature the resin from the ethereal solution was to the last softer, and contained more air-bubbles than that from the alcohol. Both were of a brilliant red colour, contracted and cracked in every direction on cooling, and in fragments were exceeding electrical.

A. Of that from alcohol 10·25 grains gave $\ddot{C}$ = 27·523, and $\dot{H}$ = 5·95 grains.

B. Of that from ether 11·65 grains gave $\ddot{C}$ = 31·177, and $\dot{H}$ = 6·987 grains.

These are equal to

	A.	B.
Carbon	= 74·247	= 73·998
Hydrogen	= 6·450	= 6·663
Oxygen	= 19·303	= 19·339
	100	100

These results agree very nearly with the formula C_{40} H_{21} O_8, which gives

40 Carbon	= 3057·480	= 74·218
21 Hydrogen	= 262·091	= 6·362
8 Oxygen	= 800·000	= 19·420
	4119·571	100

3. The close accordance of these results with a formula containing eight of oxygen, induced me to return to the *reed* dragon's blood, with the view of ascertaining whether the two varieties were really unlike in constitution, or whether the differences obtained by analysis might not result from the mode in which the resin was extracted,

or the greater or less quantity of the solvent (alcohol or ether) they were permitted to retain. A portion of it therefore was digested in ether, and the solution evaporated.

A. Evaporated and kept for twelve hours at 150° FAHR., 12·31 grains gave $\ddot{C} = 32{\cdot}606$, and $\dot{H} = 6{\cdot}757$.

B. Kept for twelve hours at 190° FAHR., 11·12 grains gave $\ddot{C} = 29{\cdot}563$, and $\dot{H} = 6{\cdot}195$.

C. Of another portion evaporated at 212°, and kept at this temperature for six hours, 11·97 grains gave $\ddot{C} = 31{\cdot}685$, and $\dot{H} = 6{\cdot}44$.

These results are equivalent to

	A.	B.	C.
Carbon	= 73·240	= 73·512	= 73·193
Hydrogen	= 6·099	= 6·190	= 5·978
Oxygen	= 20·661	= 20·298	= 20·829
	100	100	100

By this prolonged heating of the resin obtained from the pipe or *reed* dragon's blood (as it is called in commerce), by means of ether, it is made to approximate nearer in constitution to the resin from the lump dragon's blood than that which was first analysed. Still there is a deficiency of nearly one per cent. in the carbon, and a considerable deficiency also in the hydrogen, compared with what is required by the formula $C_{40}\,H_{21}\,O_8$ as above given, and which is indicated by the analysis of the lump resin.

What is the cause of the difference, then, and which of the series of analyses is to be considered as representing the true constitution of the pure resin? I place most confidence in the series (2.), for these reasons: *first*, the lump resin is mixed with seeds and fragments of vegetable matter, and is therefore most probably the resin as it exudes from the tree: *second*, the results of the analysis of this variety give an exact formula analogous to those by which so many other resins are represented, and of which the constant C_{40} is a member; and *third*, the reed dragon's blood, from its porosity, its freedom from foreign substances, and its varying colour (from a bright red to a dirty brown), is evidently a *manufactured* article, the constitution of which therefore is not to be depended upon. Since I have been led to this conclusion, I have learned that a third variety of dragon's blood is brought to the market under the name of *strained*. Now to be strained the natural resin must be made very fluid, and can hardly fail to be more or less decomposed by the temperature necessary to produce the complete fusion. The *reed* variety is probably only the *strained*, made while soft into small rolls and wrapped up.

Conclusions.—The following therefore seem to be the conclusions to which the above experiments lead.

1. That the lump dragon's blood is the natural and pure resin, the strained and reed varieties being manufactured articles, and more or less decomposed.

2. That this resin retains alcohol and ether, as most other resins do, with considerable obstinacy, but that these solvents may be entirely expelled by a long-continued (ten or twelve hours) exposure to a temperature not higher than 200° Fahr.

3. That when thus perfectly dried it is represented experimentally by the formula $C_{40} H_{21} O_8$.

The two former of these results are of considerable practical importance in an inquiry into the constitution of the resins. Several of those resins which are of the greatest value in a commercial point of view, are said to be subjected to processes of manufacture, by the native collectors, before they are brought into the market; it is therefore of consequence to ascertain that the substance is procured in its unaltered state before it is subjected to analysis. I shall have occasion, in a subsequent part of this inquiry, to mention several resins, the alteration of which by manufacture or adulteration has caused me much loss of time, and, in some cases, prevented me from obtaining a formula on which much reliance is to be placed.

In regard again to the total expulsion of the alcohol or ether, so necessary as solvents of this class of substances, if we evaporate the resins in *mass*, and heat till all the alcohol is driven off, we may often produce decomposition before our purpose is accomplished. It is therefore of great importance to have learned, that long exposure of a thin film of the resin to a low temperature will fully deprive it of those volatile liquids.

Durham, March 12, 1839.

X. *On the Male Organs of some of the Cartilaginous Fishes.* By JOHN DAVY, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

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IN a paper on the Torpedo, which was published in the Philosophical Transactions for 1834, I have briefly described the male generative organs of this fish as consisting of two firm oval testes, of vasa deferentia without vesiculæ seminales, and of a papilla opening into the cloaca, the common termination of the seminal and urinary passages.

Referring to Dr. MÜLLER's able work, "De glandularum secernentium structura penitiori," in which he treats of the testes of the Rays and Sharks, I find that his descriptions and views of these organs are not in accordance with the above. His words are, "Maxime singularis est genitalium masculorum in Rajis et Squalis conformatio; sunt enim organa glandulosa duplicis generis, altera, quæ hucusque tanquam testiculi descripta sunt, ex globulis, non vero ex ductibus seminalibus conflata, altera, plerumque pro epididymidibus habita, ex canalibus serpentinis composita, sed minime cum testiculis globulosis conjuncta; quare non epididymides sed glandulas proprias esse conjicio*."

He adds, "Organum alterum recte ab Ill. CUVIERO jam descriptum est. Dicit enim CUVIERUS, 'Ils sont grands, alongés, quoique larges et plats et s'étendent sous l'épine au-dessus du canal intestinal et de l'estomac. Leur plus grande partie est une agglomération de tubercles de la grosseur d'un pois, pressés les uns contre les autres, et présentant chacun un petite enfoncement au milieu de leur face externe. Ils tiennent ensemble par des filaments très-forts, et par la membrane extrèmement délicate, qui les enveloppe, et ne paroissent composés que d'un grand nombre de petits grains ronds très fins. L'autre partie de ces testicules singuliers est formée d'une substance glanduleuse homogène, qui en occupe en arrière, la portion la plus mince, et s'étend sous toute la face inférieure de la portion tuberculeuse†.'"

After alluding to the similar observations of TREVIRANUS on the same organs in the *Squalus acanthias*, and mentioning in confirmation those which he himself had made on the Ray, he comes to the conclusion that the bodies which hitherto in these fishes had been called epididymides, having no connexion with the testes, at least yet discovered, are glands of a peculiar kind; and he conjectures, (stating, however, that it requires confirmation,) that the globular organs are the true testes, and that the spermatic fluid secreted by them, as in the instance of the Petromyzon and the Eel, instead

* Opus cit., p. 105.

† Leçons d'Anat. Comp. tom. v. p. 27.

of flowing through an appropriate canal, may burst into the cavity of the abdomen, and be discharged by the abdominal apertures*.

Such high authority necessarily led me to doubt the correctness of my own observations, and to form the wish to repeat and extend them. This I have been able to accomplish; and I now propose to myself the honour of submitting the results to the Royal Society.

1. *Of the Male Organs of the Torpedo.*

Two good specimens of Torpedo, from the Mediterranean, of middle size, one an Oculata (*T. oculata*), the other a Tremola (*T. diversicolor*), belonging to the Museum of Natural History at Fort Pitt, have enabled me to institute a careful examination of the organs in question of these fishes. The result has accorded perfectly with my first observations made at Malta on the same species in the fresh state.

The testes appeared as I have briefly described them, both in relation to form, situation, and consistence. They exhibited indistinctly the globular structure described by Baron Cuvier in the passage quoted. In neither instance could I discover any traces of the supplementary or attached soft glandular structure noticed by Cuvier. The epididymides [the parts commonly so called] were comparatively large and distinctly tubular, terminating inferiorly in large tortuous vasa deferentia, which proceeded to the papilla or rudimentary penis within the verge of the anus, and superiorly were connected with the testes by a small number of vasa efferentia, applying the term hypothetically.

To this connexion, of course, my attention was particularly directed. The appearances were very satisfactory; small tubes or ducts could be clearly seen, passing from one to the other, and entering into the body of each. They were made perfectly apparent by means of minute dissection under water, and the immersion of the organs in dilute sulphurous acid, which has a property extremely useful in researches on minute structure, of imparting transparency to cellular tissue and serous membranes, and of expanding at the same time tubular and vascular parts.

* The following is the passage at length. "Jam vero nunc evictum credo, epididymidem sic dictam, cujus conjunctionem cum testiculis nemo hactenus vidit, omnes potius supposuere, ne vero glandulam proprii generis esse. Quæritur nunc, utrum organorum testiculus sit, ac, si semen in globuloso testiculo paratur, quomodo semen excernatur. Posset aliquis seminis secretionem in organo altero ex canalibus composito ponere. Sed organon globulosum tam peculiare magnumque est, ut cum alio quopiam organo comparari nequeat. Tum vero meminimus structuram in Anguillis et Petromyzonibus testiculorum globulosam, in quibus semen non per ductum proprium evehitur, sed, uti ova in abdomen defluens, per orificium simplex excernitur. Quæro num etiam in Rajis et Squalis semen ex globulis in cavum abdominis propullulet et orificiis illis evehatur, quæ tam in masculis quam feminis Squalis et Rajis obveniunt? licet in feminis ova proprio duplici oviductu evehantur. Hoc observationes ulteriores evincere debent. Incertum etiam manet, cujus naturæ sit secretio alterius permagnæ glandulæ, utrum maximum momentum in hocce potius organo positum sit, an glandula testiculis succenturiata sit. Certe liquor glandulæ copiosissimus alius longe naturæ est ac testiculorum globulosorum et a glandula illa ipsa secernitur." He adds, "Itaque vera epididymis in piscibus non adest."—Op. cit. p. 107.

2. *Of the Male Organs of the Thornback* (*Raia clavata*).

On the 12th of October I had an opportunity, under very favourable circumstances, of examining a male Thornback, of large size, shortly after being caught. This being the breeding season of this Ray, its generative organs were fully developed, and every part of them was peculiarly distinct and large,—as the globular organs, considered by Professor MÜLLER as testes; their soft milt-like appendage, attached to their inferior extremity, and partially bordering their inner margin; the massive epididymides, conjectured by Professor MÜLLER to be glands of a peculiar kind; and the vasa deferentia, tortuous, of capacious dimensions, terminating in a kind of urethra, close to the necks of the two sacs, which I believe perform the double function of urinary bladders and of vesiculæ seminales.

The structure of the milt-like appendages, of the globular testes, and of the epididymides, was in accordance with Baron CUVIER's and Professor MÜLLER's description of them. In neither of the two former could I observe any appearance of tubular structure, which was very strongly marked in the last.

As in the instance of the Torpedo, I made careful search after a connexion between the testes and epididymides; and using nearly the same means, I was able to satisfy myself that such a connexion exists, and in the same situation, namely, between the superior extremities of the two parts, where the space separating them is inconsiderable. The tubes of connexion, however, were smaller, and more difficult of demonstration than the analogous ones of the Torpedo.

As the vasa deferentia were distended with a cream-like fluid, which had very much the appearance of the spermatic fluid, it appeared probable that some satisfactory evidence might be obtained by instituting an examination of the contents of the different parts; and that it would be best effected by means of the microscope. The instrument I used was one of Mr. Ross's construction, provided with an achromatic object glass of one-eighth of an inch focal distance.

First, the fluid contained in the sacs, which I suppose to perform the double function of urinary bladders and of vesiculæ seminales, was submitted to examination. It was of the appearance and nearly of the consistence of cream. Under the microscope it was found to abound in animalcules in active motion, mixed with globules of different sizes. They were best seen when the fluid was diluted with a solution of common salt. The animalcules were proportionally of great length, not unlike portions of fine hair; one extremity was of extreme fineness, and seen with difficulty. Their motion was serpentine and vibratory, and of great velocity when most active, especially that of the tapering part; and their progressive motion was unquestionable, the effect of their own powers, independent of currents.

Next, the fluid from the vas deferens was examined, taken from its commencement, just after leaving the body of the epididymis. Its appearance to the naked eye, and its character under the microscope, were very similar to those of the preceding. It

abounded in the same animalcules, also in active motion, many of them grouped together in bundles, and which so joined side by side acted together, the tapering part, in which the approximation was greatest, moving with great velocity in a vibratory manner.

Lastly, I examined the fluid yielded by the globular testis, procured by making an incision into its substance, and gently scraping the cut surface. The small quantity of fluid thus obtained was opaque, but not so thick as the last. Under the microscope it was found to contain animalcules similar to the preceding, and in motion, but less numerous, and not grouped, intermixed with small globular masses of an obscure granular and radiated structure.

3. *Of the Male Organs of the common Skate* (*Raia batis*).

On the 8th of November, under the same favourable circumstances as the preceding, I examined a male fish of the above species, in which the generative organs were fully developed, and as far as I could observe generally, were in no respects essentially different from those of the Thornback.

In this instance, for the sake of as much accuracy as possible, I began with an inspection of the fluids likely to throw light on the functions of the generative organs.

The fluid first subjected to the microscope was some contained in the cavity of the abdomen, transparent, with a small opaque sediment. It was found to contain globules of different sizes, the largest less than common pus-globules, and a few elliptical blood particles, without any animalcules.

The fluid next collected was that contained in the urinary bladders. It was nearly colourless and limpid. Under the microscope many small globules were visible in it, about one half the size of pus-globules, and a few animalcules, resembling those of the Thornback, and yet not precisely similar.

On pressing the vasa deferentia, where they pass on the inner side of each urinary bladder, a cream-like fluid was discharged into the cloaca through the papilla, the termination of the urinary, and, as I believe, seminal ducts. This fluid under the microscope was found to abound in animalcules, mixed with a few globules, both similar to those last mentioned; the former in active motion.

Next, the vas deferens was opened into before it passes behind the urinary bladder, and some fluid was obtained from it, not inconsiderable in quantity, and of the colour nearly and consistence of cream. Under the microscope it was found to abound in animalcules of a thread-like form, having one extremity excessively fine, very active in movement; their motion vibratory, as well as progressive; in every respect closely resembling those of the Thornback; and like them, owing to their vast number and being intermixed with many globules, to be seen distinctly the fluid required to be diluted.

The globular testis was next cut into; a portion of it was removed by a horizontal incision. It abounded in fluid, more liquid than that of the vas deferens, and less

opaque. Under the microscope many animalcules were seen in it, precisely similar to those of the vas deferens, but not in motion, as if dead; and mixed with them were many globules of different sizes, the largest about the size of pus-globules, a few blood corpuscles, and some fragments of irregular form.

Lastly, an incision was made into the lower non-globular milt-like part, bordering the globular testis. It abounded in thick opaque fluid, of which a sufficient quantity was collected for examination by gentle pressure. Under the microscope it was found to contain a large number of globules of about the size and general appearance of pus globules, a few very much smaller, and a few animalcules, less distinctly formed than those in the globular portion and in the vas deferens, but clearly of the like kind.

The anatomical examination of the organs was next entered on. I have stated that at first view they appeared generally not to differ from those of the Thornback; a minute inspection confirmed this. A tubular connexion was found between the head of each testis and epididymis, not admitting of doubt; the tubuli were traced from the globular substance into the mass of the epididymis.

On the 24th of November another fish of this species was procured, in which also the male organs were in a very favourable state for examination, and which were examined with great care, having in view the doubtful points. No fluid in this instance was contained in the abdominal cavity; not a single drop could be collected.

As the globular and the milt-like testes, both of this Ray and of the Thornback, are connected with the epididymides by a delicate peritoneal covering, leaving a cavity on each side of the spine between the testis and epididymis, which descends close to the bladder, it occurred to me as possible, although not probable, that this cavity might be a channel between the respective testes and the cloaca. To endeavour to determine this, a small opening was made into the cavity, and it was forcibly distended with air by means of the blow-pipe, but no orifice inferiorly could be detected; the air was completely confined.

This done, the fluid contents of the different parts were subjected to microscopical examination in the following order; first, that of the urinary bladders; secondly, that of the milt-like testis; thirdly, that of the globular testis; and lastly, that of the vas deferens. The results in part were somewhat different. The fluid contained in the two urinary bladders amounted to four-fifths of a cubic inch; like the former it was colourless and transparent, with a very slight sediment. It contained a few globules, but no animalcules. The milt-like part of the testis yielded but little fluid; a minute quantity of it, collected by scraping gently an incised surface, exhibited under the microscope no animalcules, but many well-defined globular particles of different sizes, commonly smaller than pus-globules, and a few blood-corpuscles. The globular portion of the testis abounded in fluid of a creamy appearance, containing many animalcules mixed with globular particles; the former precisely similar to those observed in the first instance, and like them motionless. The fluid of the vasa deferentia, which flowed out on opening into them, amounted to seven-tenths of a cubic

inch. It had the same character as that procured from those canals in the other specimen, and abounded in animalcules in every respect similar, and like them in active motion.

The anatomical examination, too, in this instance afforded a result precisely similar to the preceding; a connexion by means of tubuli was discovered between the head of each testis and epididymis, and nowhere else, after very careful search. The whole of the generative organs, with the urinary associated with them, were removed entire, and were immediately examined under spirit of wine.

4. *Of the Male Organs of Scyllium Edwardii.*

This is the only species of Shark, the male organs of which recently I have had an opportunity of examining; it was brought by Dr. Andrew Smith from the Cape of Good Hope; and to this gentleman I am indebted for permission to inspect it. The parts were not in the best condition; they had suffered from keeping, especially the testes. The epididymides were large; as were also the vasa deferentia, which terminated, as in the foregoing instances of the rays, in a kind of urethra, connecting the urinary bladders with the cloaca.

Having found that the spermatic animalcules of the Mammalia are but little liable to change, that they may be detected even in putrescent fluids, and may be kept for a long time in spirit of wine, I thought it worth while to examine with the microscope any fluid that might be found in the vasa deferentia of this Scyllium. A little turbid fluid mixed with grumous matter was procured by laying them open, in which, when diluted, animalcules were distinctly seen, resembling much those of the Rays.

This result induced me to try the contents of the testes and vasa deferentia of the Torpedo. The experiment was made with two fishes, varieties of the Tremola, rather below the middle size, which I had sent home from Malta, now more than four years ago preserved in spirits. The vasa deferentia of both were large; and when opened afforded pretty much opaque, white, semifluid matter. When diluted with water and placed under the microscope, animalcules were distinctly visible in it; thread-like, serpentine, finely tapering towards one extremity, very similar to those of the common Ray and Thornback, but decidedly smaller; and animalcules of precisely the same kind, but less numerous, were detected in the semifluid opaque matter obtained in minute quantity by scraping gently the cut surface of the testes. The animalcules in the testes and vasa deferentia of both specimens offered no perceptible difference.

5. *Of the Accessory Male Organs.*

Before entering on the inferences to be drawn from the foregoing observations relative to the functions of the different parts constituting the male organs which are contained within the abdominal cavity, I would wish to offer a few remarks on the external accessory organs, which have commonly been considered auxiliary to the more important internal ones.

They are the anal appendages, the characteristic of the male cartilaginous fishes, organs of complicated and curious structure, the use of which even at present is far from being understood.

The Torpedo, the common Ray, and the Thornback are the only species of Rays which I have yet carefully examined in relation to the organization of these parts. In each species they are very similar, consisting of articulated bones, muscles, mucous glands, mucous ducts, &c., and containing a large and remarkable gland associated with an elaborate and complicated structure. On account of the large size of the common Ray and its large anal appendages, and their full development, the gland and its accompaniments are seen in this fish to great advantage. In two specimens of *Raia batis* which I have examined, the gland was nearly the size of a chestnut, of a very elongated oval form, divided on one side, as it were, into two columns by a furrow or depression, in which were two rows of delicate projecting tubuli, the extremities of its excretory ducts. The substance of the gland was enveloped in a muscular coat, and this was covered with a vascular tissue. The gland itself was contained in a sac composed of three coats, an inner fibrous coat, a middle muscular, and an outer cellular one; and was surrounded with strong muscles, the principal flexor and extensor muscles of the organ*.

Moreover, at the inferior extremity of the sac, just below its outlet, there was a distinct cavity, formed of muscular walls and intersected by delicate tendinous fibres. In one instance, when under examination, the fish was still irritable, its muscles acting when stimulated, and then this part pulsated regularly and vigorously. It contained blood: I believe it to be an auxiliary heart, designed for circulating the blood in the appended organ. A similar structure exists in the same situation in the Thornback and Torpedo.

In the sac of the gland a cream-like secretion was found, and the same flowed out pretty copiously through the excretory ducts when pressure was applied to the gland. It was neither acid nor alkaline; it was slightly viscid; applied to the tongue no sensation was immediately produced, perhaps there was an after one approaching to acrid, but so slight as to be doubtful. Under the microscope it was found to abound in very minute, dense, spherical particles, twenty of which, at least, would be required to form a mass equal to a blood corpuscle of man. They had no appearance of independent vitality, and moved only when in currents.

The blood in the pulsating cavity, from which it is probable that the secretion just mentioned is formed, coagulated like ordinary blood on exposure to the air; but it was more dilute; and, what is remarkable, under the microscope its particles appeared to be smaller, and the majority of them not elliptical but globular.

* Bloch in his description of these organs describes only two muscles; but there are more, some connecting the organ with the pelvis, others attached to the principal bones, and others to the smaller bones; which is, as might be expected, considering that eleven bones and one terminal cartilage enter into the composition of each organ. This number I found in the instance of the Thornback; Bloch states, that in this fish they are eight (Hist. Nat. des Poissons, iii. 672.).

How the anal appendages are constituted in Sharks I cannot speak from my own observations, having yet examined these organs only in one instance, that of *Scyllium Edwardii*, before referred to. From the descriptions of naturalists it may be inferred, that they vary more or less in organization in different genera; that in some, as probably in the genus Carcharias, there is a distinct gland, secreting an opaque fluid, similar to that of the Rays I have mentioned; but in others, as in the genus Scyllium, the gland is wanting, and its place is supplied by a sac, one for each organ, situated under the common integument of the lower part of the abdomen, communicating by a narrow elongated passage with the appendage*, and containing a slightly viscid fluid†, probably secreted by follicles situated between the fibrous inner coat and its outer muscular one. This structure I am informed by Dr. Andrew Smith, who it would appear first observed it, occurs in every species of the genus; and, as in one specimen, that of *Scyllium Edwardii*, the gland is not to be found (we carefully sought for it together), it is probably deficient in the others.

6. *Concluding Remarks.*

As in the Torpedo, the common Ray, and the Thornback, a tubular communication has been found to exist between the globular testes, and the bodies hitherto called epididymides; as the fluid in the former was found to be similar to that of those canals, continuations of the epididymides, which have commonly been considered as vasa deferentia; as this fluid, in its entozoa, possesses the essential character of a spermatic fluid; and, lastly, as it could not be detected free in the cavity of the abdomen, may it not be inferred, and is not the conclusion unavoidable, that the old opinion respecting the functions of these different parts is correct, and in accordance with the names which they have received?

The evidence on which this conclusion is founded is manifestly of two kinds; one anatomical, the other microscopical, connected with the constitution of the spermatic fluid. Preparations have been made of the parts showing the tubular connexion, which are deposited in the collection of comparative anatomy belonging to the museum at Fort Pitt.

Relative to the evidence derived from the nature of the spermatic fluid, I apprehend, now, it will generally be admitted as satisfactory. I have examined the spermatic fluid of many species of Mammalia, and the animalcules contained in it were in no instance more distinct than those of these fish; indeed in the latter they were perfectly so, and their character well marked. They were immediately killed by spirit of wine; they were torpid and motionless in a saturated solution of common salt; they became active when the solution was diluted; and in the unmixed fluid they

* It communicates with the outer surface of the appendage by an opening close to the anal membrane, and also with the groove or tube of the organ, and thus with its internal surface. The one opening is not continuous with the other as in the Rays, a space equal to about a quarter of an inch intervenes.

† According to Dr. Smith, the fluid contents are like very dilute white of egg.

retained life for three or four days after removal from the fish, until signs appeared of incipient putrefaction.

It is true that Sir EVERARD HOME, in the fifth volume of his Lectures on Comparative Anatomy, has denied that spermatic animalcules essentially belong to the prolific fluid, and was of opinion that they are a mere fiction of the mind, because neither he nor Mr. BAUER could detect them. His opinion at the present time, I apprehend, can have no weight, and it will be received only as a proof of the imperfection and feebleness of the instrument which he used, and serve as a warning, much as it is to be regretted, that no reliance is to be placed on his microscopical observations.

The testes of the Torpedo are distinguished from those of the Thornback and common Ray, by wanting that milt-like margin or appendage described by Baron CUVIER. Should it be found wanting in all the viviparous species of cartilaginous fishes, its presence in the oviparous may be considered as a link between them and the osseous oviparous fishes furnished with milts*. Perhaps in the milt-like part of the testes of the Ray and Thornback, the ova of the spermatic animalcules are developed; and, perhaps, in the globular portion they grow and are matured. The microscopical observations described seem to be rather in favour of this idea.

If the view which I have advocated of the functions of the testes and of the epididymides be received, the structure of the subordinate generative parts within the cavity of the abdomen, will, I believe, be found to be in perfect accordance, and to offer fresh evidence in favour of its correctness: I now allude to the vasa deferentia and the part or parts inferiorly connected with them.

The vasa deferentia in the three Rays which have been noticed are similar, and well adapted both to conduct and to hold in reserve such a fluid as the spermatic, for they are comparatively large and tortuous, and are provided with circular valvulæ conniventes, forming a vast number of cells. Both in the Ray and Thornback, and in the Torpedo, they terminate in what may be considered the urethra of a rudimentary penis, the end of which projects into the cloaca in the form of a papilla. In the Torpedo, which differs from the other two in having no urinary bladder, the spermatic and urinary ducts terminate near the mouth of the papilla, by separate and very minute orifices. In the Thornback and Ray, which are possessed of two urinary blad-

* The spermatic fluid of the milt of the bony fishes bears some resemblance to the fluid of the milt-like portion of the testes of the Rays. I have examined it in the Herring, Smelt, Cod, Dab, and Pike; in the three first it appeared minutely globular, and no filamentous prolongations were distinctly visible in the animalcules; in the Pike a single filament was seen attached to many of the globules, and in the Dab, two. MM. DUMAS and PREVOST state that the spermatic animalcules of all the fishes they had examined are filamentous, and that the filament or prolongation had previously escaped observation on account of its great tenuity; an opinion I can readily adopt, as I could perceive the prolongations of the animalcules of the Dab and Pike only in a very favourable light, and by means of very nice adjustment. What the species of fishes were examined by these gentlemen is not noticed; their remark on the subject is made incidentally in their ingenious memoir on the spermatic animalcules of many of the vertebrata, with the promise to give the details on a future occasion (Ann. des Scien. Nat. tom. i. and ii.).

ders, these ducts terminate near the fundus of the short urethra, and close to the neck of each bladder, or its opening into the common passage*. These bladders appear to be somewhat analogous to the bifid bladder of Frogs and Toads, the latter connecting them with the simple urinary receptacle of the higher vertebrata.

I have expressed the opinion that these organs may perform the double function of vesiculæ seminales and of urinary bladders, partly from their situation and connexion, and partly from their contents. I have mentioned, that in one instance, I found in them some spermatic animalcules, which may be considered equivalent to spermatic fluid. The result of a chemical examination of one specimen of the fluid which was contained in them, amounting nearly to a cubic inch, was favourable to its urinary character; for besides affording a little saline matter, principally common salt, it yielded a little animal matter very analogous to urea, soluble in alcohol, uniting with nitric acid, and the compound crystallizable and soluble.

The nature of the anal appendages and their functions, have from the earliest times of natural history been more or less a subject of controversy. Aristotle considered them as characteristic of, and peculiar to, the male cartilaginous fishes. Lorenzini erroneously denied that they are distinctive marks of the sexes. Willoughby, Ray, Artedi and Broussonet considered them as organs of intromission, as penes. Rondellet opposed this notion, and considered them as holders, in which opinion he was followed by Bloch, who, I believe, was the first to describe them with tolerable accuracy and minuteness†. Recently, some naturalists appear to have adopted one conclusion, some the other. The majority favour the idea of Bloch, that they are analogous, as he ingeniously endeavoured to prove, to arms or rather feet, and intended for seizure and holding fast. A small number, amongst whom M. de Blainville is eminent, seem to have returned to the older notion that they are penes. Bloch's argument against their being penes, is founded on their structure, in his opinion totally unfit for their supposed office. Those who maintain the opinion he opposes, lay stress on the appearance of the fluid secreted by the gland belonging to each appendage, so much resembling the spermatic fluid, and on the analogy in certain of the reptiles of a double oviduct and double penis.

Objections, it appears to me, are unavoidable to both these views. As regards the latter, it is highly improbable that there should be two sources of spermatic fluid: moreover, it has appeared that the fluid of the appendages is without the characteristic quality of the spermatic fluid, containing no entozoa, and seeming to be of a peculiar kind. As regards Bloch's view, it seems improbable, were the appendages de-

* Delicate probes passed through the bladder, ureter, and vas deferens of the common Ray and Thornback meet at the bottom of the short urethra, common to both sides; the openings into it are small and contracted; the vas deferens on each side terminates in it by a projecting papilla, having an oblique direction. I have carefully sought for a termination of these ducts into the bladder itself, but in vain. I mention this particularly, because a passage in the valuable Descriptive Catalogue of the Museum of the Royal College of Surgeons would seem to imply such a termination (vol. iv. p. 51. No. 2394.).

† Hist. Nat. des Poissons, iii. p. 672. His description was confined to these organs in the Thornback.

signed for prehension, that they would be furnished with a gland copiously secreting a lubricating fluid: nor does their general structure or situation appear to be apposite for the purpose he imagines, especially as he is of opinion that the rudimentary penis is applied in the act of impregnating to the surface of the cloaca, where the mouth of the oviducts or uterine cavities open.

Reflecting on the subject and on the inadequacy of former hypotheses, it has occurred to me as possible, that these organs may be designed for intromission and retention like the penis of the Dog. On this idea, the abundant secretion of the gland, as a lubricating fluid, would be of manifest use, and the action of the different parts might admit of explanation. And, as the sacs connected with the appendages in certain of the Sharks open appropriately, they may be supposed to be designed for the same use*. In venturing to bring forward this conjecture, I beg to be understood, that I attach no kind of importance to it. I am fully aware that it is liable to objections. I shall be satisfied should it lead to further inquiry, by which alone the true use of these mysterious organs can be determined.

* After writing the above, on referring to Aristotle, that great and curious storehouse of natural history, I found that a similar idea had been entertained in his time, and that there were some who declared they had witnessed the fact. "Sunt qui se vidisse confirment nonnulla ex cartilagineis aversa modo canum terrestrium cohærere." De Hist. Animal. L. v. c. v.

Fort Pitt, Chatham,
Dec. 22, 1838.

XI. *Researches on the Tides.—Tenth Series. On the Laws of Low Water at the Port of Plymouth, and on the Permanency of Mean Water.* By the Rev. W. Whewell, *B.D. F.R.S., Fellow of Trinity College, Cambridge.*

Received April 11,—Read June 6, 1839.

In former communications to the Society, the laws of high water at Plymouth and other places have been the subject of my researches. These being obtained, the laws of low water are a subject of importance and interest on many accounts. The first ground of my pursuing this subject was the desire to ascertain how far the *mean water*, that is, the height midway between high and low water, is permanent during the changes which high and low water undergo. That it is approximately so at Plymouth, had been ascertained both by Mr. Walker and myself, by means of a comparison of a short series of observations. But it was desirable to know with more exactness what was the real amount of this permanency, when, by using a long series of observations of high and low water, the irregularities arising from accident, and from taking imperfect cycles of inequalities, were eliminated.

There was another reason which made this inquiry important at the present time. An operation has been recently carried on by the direction and at the expense of the British Association, with a view of ascertaining what surface ought to be taken as the permanent level of the sea. A Level Line has been carried with great care and accuracy from the north shore of Somerset to the south shore of Devon; and the position of this line has been fixed, so as to be recognised at any future time, by means of marks* at Axmouth, at East Quantockshead, at Stolford, and at Portishead. This line has also been referred to the sea at its extremities; and the observations show that the height of mean water coincides, at least very nearly, at different places, as well as at the same place at different times. While the difference of levels of low water at Axmouth on the English Channel, and Wick Rocks on the Bristol Channel, is not less than twelve feet; the mean water at those two places coincides in level within a few inches. In order to determine further what accuracy may be attained

* These marks, and their respective heights above a certain arbitrary level, are as follows:

	Height.
Copper bolt in a granite block at Axmouth	83·6513 feet.
Copper bolt in a granite block at East Quantockshead	244·4365 feet.
Copper bolt in a granite block at Stolford	125·1114 feet.
Iron bolt in the rock at Portishead	102·5795 feet.

The account of the operation of carrying the Level Line to these different points, and comparing its position with that of the surface of the sea, will appear in the Transactions of the British Association for 1838.

in this result, we are led to inquire what is the degree of permanency at one place. I may further add, that it cannot but be instructive to know how far the corrections of the height and time of low water, for lunar parallax and declination, agree in form and amount with the same corrections already obtained for high water.

I took, therefore, six years of observations at Plymouth (1833—1838), made, as I had reason to believe, with care and accuracy under the superintendence of Mr. WALKER, at present the Queen's Harbour Master at that port: and I had them discussed by Mr. Ross of the Hydrographer's Office at the Admiralty; by which gentleman, on this as on former occasions, the requisite calculations have been performed with much zeal and intelligence.

The method employed in discussing the observations was the same, with slight modifications, as in former researches. The low waters were referred to a transit of the moon anterior by about two days to the time at which each occurred; and according to the hours of these transits, were divided into twelve horary groups, from 0^h to 1^h, from 1^h to 2^h, and so on. In order to find the laws of the heights, the mean height was taken for each of these groups. The mean parallax for each group was very nearly the absolute mean lunar parallax; and the mean declination for each group differed from the absolute mean by a small quantity, according to a known law. Hence the mean heights of the separate horary groups, compared with one another, gave the law of the height as depending upon the hour of transit; that is, they gave the semimenstrual inequality of height for low water.

1. *Of the Permanency of the Height of Mean Water.*

The height of low water, cleared of the effects of lunar parallax, and very nearly of the effects of lunar declination, and compared with the height of high water similarly cleared, enabled me to ascertain whether the mean water also was affected by a semimenstrual inequality. The following are the results of this calculation, keeping the six successive years separate.

Height of Mean Water at Plymouth.

Transit	h m 0 30		h m 1 30		h m 2 30		h m 3 30		h m 4 30		h m 5 30		h m 6 30		h m 7 30		h m 8 30		h m 9 30		h m 10 30		h m 11 30		Mean.	
	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.	ft.	in.
1833.	9	10¾	10	0	10	1¾	10	3¾	10	5½	10	6½	10	6	10	4¾	10	3¾	10	1¾	9	11	9	10½	10	2·5
1834.	10	0	10	2	10	3	10	4½	10	6	10	7	10	6	10	2½	10	1	10	0½	9	11½	9	10½	10	2·6
1835.	10	1	10	1	10	1½	10	3½	10	4½	10	6	10	5½	10	3	10	1¾	10	0½	10	0	10	0½	10	2·4
1836.	10	1½	10	2½	10	4	10	6½	10	7	10	8½	10	6½	10	4½	10	3½	10	1½	10	1½	10	0½	10	4·0
1837.	10	0	10	1	10	2	10	4	10	6½	10	6½	10	5	10	2½	10	1½	10	0	10	0	9	11	10	2·3
1838.	10	4	10	4½	10	6½	10	8	10	8½	10	9	10	8¾	10	5½	10	5½	10	4	10	4½	10	4½	10	6·1

It appears from this table that the height of mean water is constant from year to year within two or three inches.

It appears also that the mean water for each fortnight has a semimenstrual inequality amounting to six or seven inches; the height of the mean water being greatest

when the transit is at 6^h, and least when the transit is at 12^h. The immediate cause of this inequality of the mean water is, that the semimenstrual inequality of low water is greater than that of high water; as I shall soon have further occasion to remark.

How far this small semimenstrual inequality of the height of mean water is universal for all places, I am not at present able to pronounce. But I am strongly disposed to believe that the difference in the amount of the semimenstrual inequality of high water and of low water depends upon local circumstances; and therefore that the semimenstrual inequality of the mean height is a casual and partial result; the general rule being that the mean height is constant, except so far as it is slightly modified by local circumstances.

2. *Of the Semimenstrual Inequality of the Height of Low Water at Plymouth.*

The height of low water is affected by the moon's declination, and hence the mean height of low water for a year depends upon the mean declination. Now the mean declination for the year is different in successive years in consequence of the change of position of the moon's orbit. Hence the mean height of low water will be different in successive years. The same may be said of high water. The following is the comparison of the successive years now under discussion with reference to this circumstance.

Mean Annual Low Water and High Water at Plymouth, compared with the Mean Lunar Declination.

	1833.	1834.	1835.	1836.	1837.	1838.
Mean declination .	14° 14′	15° 17′	16° 15′	17° 12′	17° 37′	17° 50′
	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
Low water	4 1	4 2	4 3½	4 5½	4 8½	4 7½
High water.....	16 4	16 3	16 1½	16 2½	16 2	16 4½

It appears from this, that in the mean low water there is a tolerably regular increase corresponding to the increase of declination, and amounting to about two inches for each degree of declination. In the high waters, this change is less marked. When we obtain a declination table from the observations, we find that about the middle part of this table, (from decl. 12° to 18°), the correction for declination is about one inch for each degree, which accordingly I shall adopt.

Hence the semimenstrual lines for successive years, obtained by merely taking the mean results of the year, will differ in consequence of the different mean declinations of the moon. And in order to obtain from them a table suited to the absolute mean of lunar declination, which for such purposes is about 16½°, we must correct the result of each year by a proper quantity; which quantity may, without sensible error, be supposed the same for all hours of transit, and at Plymouth will amount, as I have said, to about one inch for each degree.

If we suppose the moon to move in the ecliptic, the mean of all her simple declinations will be less than 15°; but since the declination correction varies as the square of the declination, we must take as the mean declination of the tables, that which gives the mean correction; which is about 16½°, as stated above. The mean declinations in page 153 are obtained by adding the simple declinations only. Hence it appears that the year which corresponds most nearly to the mean declination correction is the year 1833; and to this, therefore, the others will be reduced.

The semimenstrual inequality for each year will be given at the end of the paper: and for the reasons already stated, I subtract one inch from the heights for 1834, two inches for 1835, three inches for 1836, three inches for 1837, and four inches for 1838. I thus obtain the following results.

Semimenstrual Inequality of the Height of Low Water at Plymouth reduced to 1833.

Moon's Transit.	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30	Mean.
	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
1833.	2 1	2 7	3 6	4 8	5 9½	6 7	6 5	5 5½	4 3	3 2	2 4	2 0	
1834.	2 2	2 11	3 8	4 8	5 9½	6 7	6 5	5 3	4 1	3 2	2 5	2 0	
1835.	2 3	2 9	3 6	4 8	5 8½	6 7	6 4	5 4½	4 3½	3 2	2 6	2 1½	
1836.	2 2½	2 10	3 8½	4 11	6 0	6 10	6 6	5 5	4 3½	3 2	2 5½	2 1	
1837.*	2 0	2 7	3 5	4 7	5 10	6 6	6 3	5 2	4 0	2 11	2 4	2 0	
1838.	2 4½	2 11	3 10	4 11	5 11	6 8	6 7½	5 5	4 4	3 3	2 8½	2 5	
Mean..	2 2¼	2 9¼	3 7¼	4 9	5 10	6 7½	6 5	5 4½	4 2½	3 1½	2 5½	2 1¼	4 1½

In order to compare this with the semimenstrual inequality of high water at the same place, I take the mean of these heights, which I find to be 4 feet 1½ inch, and I express each height by its defect or excess with reference to this mean. In like manner, taking the table of the semimenstrual inequality of high water at Plymouth, I find the mean height to be 16 feet 3½ inches; and I express each other height by the excess or defect with reference to this. In this way I obtain two comparable expressions for the semimenstrual inequality of low and high water as follows:

Moon's transit	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
Low water	−23½	−16¼	− 6½	+ 7½	+20½	+30	+27½	+15	+ 1	−12	−20	−24¼
High water....	+16½	+12½	+ 5½	− 4	−13½	−20½	−20	−12½	− 3	+ 8	+14	+17¼

Thus it appears that the *semimenstrual inequality of low water at Plymouth is greater than that of high water* in the ratio of 3 to 2 nearly, the total amounts being respectively 53¼ inches, and 37 inches. The total semimenstrual inequality of the mean water is half the difference of those two, or eight inches nearly: but this is to be reduced in consequence of the correction for parallax.

* There is a seeming anomaly in the results of 1837, which is caused, at least in part, by employing apparent time for that year, while the others (calculated afterwards) were referred to mean time.

3. *The Parallax Correction of the Height of Low Water at Plymouth.*

The parallax correction is obtained from all years alike, by taking the residue of each observation which remains when the semimenstrual inequality is taken away, and arranging these residues (for each hour of the moon's transit) according to the parallax. The mean declination for each column of such an arrangement is very nearly the absolute mean declination for the year; and hence the different heights will depend almost entirely upon the different parallaxes. In this manner we obtain the effect of parallax, arranged according to hours of moon's transit. But as the effect upon the height is nearly the same for all hours of transit, I take the mean of all the twelve hours, and thus obtain the parallax correction for the height of low water. I place along with this the parallax correction for the height of high water at

Parallax	54′½	55′½	56′½	57′½	58′½	59′½	60′½	61′½
Low water ..	+ 8	+ 6	+ 3½	+ 0½	− 4	− 8½	−13	−15
High water..	− 7½	− 4½	− 1½	+ 1½	+ 4½	+ 7½	+10½	

the same place. Hence it appears that at Plymouth the parallax correction for height is somewhat greater for low water than for high water.

In the Appendix, where the parallax corrections for the separate hours are given, it will be seen that although the *mean* parallax correction for parallax 57′½ is very small, and may almost be taken as 0, the parallax correction for the different hours for this value of the parallax, ranges from − 4·4 inches to + 3·7 inches. This arises from the circumstance that the range of parallax at different hours is not the same, owing to the moon's *variation*. By reason of the sun's action upon her, her orbit is an oval, the smaller axis of which is in the direction of the sun. Hence at syzygy she comes so near the earth, that her parallax amounts to 61′½; but at quadratures her parallax never exceeds 59′½. Consequently the mean parallax at syzygy is about 58′, and at quadrature about 57′. Hence if we take 57′½ for the mean parallax, the semimenstrual curve, obtained as above, is affected by a parallax correction, which is − at 0^h, and + at 6^h transit. If we take away this correction, so as to obtain the true mean semimenstrual inequality, we find the following.

Semimenstrual Inequality of Low Water at Plymouth, for the Mean Parallax and Declination.

Transit	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
H. low water	ft. in. 2 7	ft. in. 2 11½	ft. in. 3 9	ft. in. 4 10½	ft. in. 5 9½	ft. in. 6 6	ft. in. 6 1½	ft. in. 5 2	ft. in. 4 3	ft. in. 3 3	ft. in. 2 8	ft. in. 2 4

The total amount of this semimenstrual inequality is now 50 inches; and as the total inequality for high water is 37 inches, the total semimenstrual inequality for mean water amounts to 6½ inches.

4. *The Declination Correction of the Height of Low Water at Plymouth.*

The declination correction is obtained in a manner analogous to the parallax correction, from each year's observations. But the correction thus obtained is that which supposes the mean declination of each year to require no correction. Now this mean declination in different years is, as we have said, different. Therefore the declination correction so calculated will be different in different years; and hence we should require different declination tables in different positions of the moon's nodes. But the semimenstrual inequality is also different in different years, in virtue of the difference of the moon's mean declination. And when we take from the semimenstrual inequalities that which is requisite to reduce them to the mean declination $16\frac{1}{2}°$, and add it to the declination corrections for each year, the declination corrections for different years coincide very nearly. For this purpose we add one inch to 1834, two inches to 1835, three inches to 1836, three inches to 1837, four inches to 1838, which were subtracted before. In this way we obtain the following results. These are the mean declination corrections, no account being taken of the difference of hours, which produces little effect.

I also add the declination corrections as obtained for high water at Plymouth. By comparison it will appear that the low water corrections are larger, especially for the high declinations.

Declination Correction. Plymouth.

Declination.....	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 27°
	in.	in.	in.	in.	in.	in.	in.	in.	in.
Low water 1833..	$-5\frac{1}{2}$	$-4\frac{1}{2}$	$-4\frac{1}{2}$	-2	$-1\frac{1}{2}$	$-\frac{1}{2}$	$+2\frac{1}{2}$	$+5\frac{1}{2}$	
1834..	-6	-7	-5	-4	-1	$+\frac{1}{2}$	$+2\frac{1}{2}$	$+7$	$+11$
1835..	-5	$-4\frac{1}{2}$	-5	-2	$-1\frac{1}{2}$	$+1$	$+3$	$+6$	$+9\frac{1}{2}$
1836..	$-6\frac{1}{2}$	-7	$-4\frac{1}{2}$	-4	-1	0	$+4$	$+7$	$+11$
1837..	$-3\frac{1}{2}$	-5	$-3\frac{1}{2}$	$-2\frac{1}{2}$	-2	$+\frac{1}{2}$	$+4$	$+6$	$+9$
1838..	$-3\frac{1}{2}$	-5	$-2\frac{1}{2}$	$-2\frac{1}{2}$	$-1\frac{1}{2}$	$+\frac{1}{2}$	$+5$	$+6\frac{1}{2}$	$+12$
Mean	-5	$-5\frac{1}{2}$	$-4\frac{1}{4}$	$-2\frac{3}{4}$	$-1\frac{1}{2}$	$+\frac{1}{2}$	$+3\frac{1}{2}$	$+6\frac{1}{2}$	$+10\frac{1}{2}$
High water.....	$+4\frac{3}{4}$	$+3\frac{3}{4}$	$+3$	$+2$	$+1$	0	$-1\frac{1}{2}$	-3	-5

There can be little doubt that the correction, as here given, for low water is more exact than that for high water, the process by which it is deduced having been applied in a more regular manner. And I may further observe, that the discussions, of which I have now been stating the results, remove all doubt on the question whether the declination correction, empirically deduced, varies as the square of the declination. The correction for low water given above, follows that law with great precision, as appears thus. The above corrections, reduced to 40ths of an inch and to declination 0, are as follows; and the squares of the corresponding declinations are expressed in the line below.

Correction . . .	10	− 10	40	100	150	230	350	470	630
Square of decl. .	1	16	49	100	169	256	361	484	625

I expect shortly to be able to give, as a Sequel to this memoir, a discussion of the *times* of low water at Plymouth corresponding to this discussion of the heights.

Postscript.

As a further proof how very nearly constant is the height of mean water, I annex the result of one year's observations made at Dundee, discussed by Mr. Dessiou. It will be seen that the differences are confined within 1½ inch, except at 11 and 12 o'clock, when they become about two inches more. This is in a (spring) tide of fourteen feet.

Dundee Tide Observations, 1837. Semimenstrual Inequality, Height of High and Low Water, and Mean Height.

Moon's Transit.	Interval between Moon's Transit and High Water.	Height of High Water.	Height of Low Water.	Height of Mean Water.
h m	h m	ft. in.	ft. in.	ft. in.
0	2 35	17 6·4	3 8·7	10 7·5
30	2 26·5	17 7·5	3 6	10 6·7
1 0	2 19·5	17 7·6	3 5	10 6·3
1 30	2 12·6	17 7·2	3 5·3	10 6·2
2 0	2 6	17 6·5	3 6	10 6·2
2 30	1 58·5	17 5·2	3 7·3	10 6·2
3 0	1 51	17 2	3 9	10 5·5
3 30	1 43·4	16 9·4	3 11·5	10 4·2
4 0	1 38	16 5	4 5·2	10 5·1
4 30	1 32·8	16 0·7	4 11·0	10 5·8
5 0	1 30·2	15 9·7	5 3·1	10 6·4
5 30	1 31·2	15 6·6	5 7·3	10 6·9
6 0	1 34·5	15 1·4	5 11·8	10 6·6
6 30	1 41·2	14 8	6 4·2	10 6·1
7 0	1 56	14 6·8	6 6	10 6·4
7 30	2 14·6	14 7	6 6·5	10 6·7
8 0	2 30	14 10·2	6 3·3	10 6·7
8 30	2 42	15 2	5 10·3	10 6·1
9 0	2 50·2	15 6	5 5·5	10 5·7
9 30	2 54	15 10	5 1·4	10 5·7
10 0	2 53·8	16 3·5	4 9	10 6·2
10 30	2 51	16 9·3	4 3·5	10 6·4
11 0	2 48	17 2	4 2	10 8
11 30	2 43·5	17 5	4 0·4	10 8·7

APPENDIX,

Showing the results of the Calculations on which the preceding Memoir is founded.

Plymouth. Heights of Low Water. Semimenstrual Line.

	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
1833.	2 1	2 7	3 6	4 8	5 9½	6 7	6 5	5 5½	4 3	3 2	2 4	2 0
1834.	2 3	3 0	3 9	4 9	5 10½	6 8	6 6	5 4	4 2	3 3	2 6	2 1
1835.	2 5	2 11	3 8	4 10	5 10½	6 9	6 6	5 6½	4 5½	3 4	2 8	2 3½
1836.	2 5½	3 1	3 11½	5 2	6 3	7 1	6 9	5 8	4 6½	3 5	2 8½	2 4
1837.	2 3	2 10	3 8	4 10	6 1	6 9	6 6	5 5	4 3	3 2	2 7	2 3
1838.	2 8½	3 3	4 2	5 3	6 3	7 0	6 11½	5 9	4 8	3 7	3 0½	2 9

	Declination. 0h 30m.									Parallax. 0h 30m.						
	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 28°	54½′	55½′	56½′	57½′	58½′	59½′	60½′	61½′
	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
1833.	+ 3·2	+ 9·5	+ 8·6	+ 2·5	− 3·3	+ 3·9	− 8·3	− 3·4		− 6·9	− 7·8	− 5·0	− 2·9	− 1·8	+ 8·2	+12·7	+14·6
1834.	+11·4	+ 6·7	+ 3·3	+ 6·4	+ 7·8	− 3·0	− 2·0	− 8·9	−11·7	− 6·5	−16·0	−12·8	− 7·3	− 2·5	+ 6·5	+10·7	+21·7
1835.	+ 9·5	+ 2·8	+ 8·2	+ 7·2	+ 1·8	+ 3·0	− 4·0	− 4·9	− 7·1	−12·9	− 3·4	− 1·8	− 2·8	+ 6·2	+11·0	+14·8	+ 9·1
1836.	+12·0	+ 9·7	+ 4·0	− 0·3	+ 3·1	+ 9·0	+ 3·1	+ 1·2	−11·6	−15·9	− 0·5	+ 5·6	− 0·5	+10·5	+10·0	+ 8·7	− 1·0
1837.	+ 5·4	+ 9·5	+11·6	+ 4·8	+ 6·6	+ 4·4	− 3·9	−10·5	+ 6·5	−12·2	− 6·9	− 7·3	− 0·5	+ 9·4	+ 8·5	+10·2	+12·3
1838.	+ 5·0	+ 8·7	+10·2	+ 5·7	+ 6·0	+ 6·0	+ 1·2	+ 1·0	− 9·3	− 2·6	−10·0	− 9·6	−12·3	− 7·5	+ 3·3	+10·8	+19·9
	+ 7·8	+ 7·8	+ 7·6	+ 4·4	+ 3·7	+ 3·9	− 2·3	− 4·3	− 9·2	− 9·5	− 7·4	− 5·1	− 4·4	+ 2·4	+ 7·9	+11·3	+12·8
	1h 30m.									1h 30m.						
1833.	+16·0	+ 1·5	+ 2·2	+ 4·2	+ 6·0	− 2·8	− 1·2	− 8·7		− 9·0	− 7·3	− 6·7	− 6·6	+ 1·7	+ 5·2	+14·6	+15·7
1834.	+ 6·0	+15·8	+ 7·5	+ 3·5	+ 2·0	+10·2	− 9·7	− 6·7	− 7·6	− 5·3	−13·1	− 5·3	− 8·3	+ 2·0	+ 2·3	+14·0	+25·3
1835.	+ 7·3	+11·5	+ 8·7	+ 0·3	+ 7·0	+ 2·0	− 7·0	− 8·8	− 6·0	−12·9	− 2·2	+ 1·5	− 1·6	+ 5·8	+13·0	+ 9·6	+12·7
1836.	+ 8·0	+11·7	+10·6	+ 9·0	+ 5·0	+ 6·3	− 2·1	− 4·3	− 9·8	−11·7	− 3·7	+ 5·3	+14·7	+ 3·6	+ 7·3	+ 4·1	+ 1·0
1837.	+ 6·2	+ 4·8	+ 7·0	+ 6·2	+ 5·7	+ 1·7	− 2·4	− 0·8	− 6·4	− 8·3	− 5·2	− 4·4	− 2·3	+ 1·5	+ 7·6	+13·3	+14·0
1838.	+ 7·0	+ 7·5	+ 1·0	+ 6·8	+11·4	+11·4	+ 0·9	+ 1·3	−10·7	− 3·6	−10·3	−15·7	− 9·0	− 7·6	+ 4·8	+17·3	+18·0
	+ 8·4	+ 8·8	+ 6·2	+ 5·0	+ 6·2	+ 4·8	− 3·6	− 4·7	− 8·1	− 8·5	− 7·0	− 4·2	− 2·2	+ 1·2	+ 6·7	+12·1	+14·5
	2h 30m.									2h 30m.						
1833.	+ 0·2	+13·7	+10·0	− 2·8	+ 2·5	+ 2·1	− 2·4	− 7·6		− 6·3	− 8·6	− 7·6	+ 2·0	+ 0·4	+ 8·8	+12·5	
1834.	+ 9·6	+ 4·0	+ 5·0	+13·0	+ 4·8	− 2·7	− 2·7	− 7·0	− 8·9	− 4·3	− 8·9	− 4·2	− 4·0	− 2·7	− 1·8	+18·6	
1835.	+10·6	+ 4·6	+11·6	+ 8·0	+ 7·4	− 1·2	− 0·4	− 5·9	−10·4	−12·7	− 0·5	0	+ 6·7	+ 1·5	+ 9·4	+10·5	
1836.	+12·5	+12·8	+ 8·0	+12·0	+ 6·2	+ 2·2	− 1·0	− 6·3	−10·7	−13·6	− 2·7	+11·0	+14·2	+ 6·8	+ 3·4	+ 1·8	
1837.	+ 8·0	+ 6·3	+ 1·6	+ 0·2	+ 3·2	+ 4·4	+ 3·2	− 1·6	− 5·9	− 9·1	− 5·6	− 1·2	− 2·2	− 1·8	+ 8·7	+15·3	
1838.	+ 0·7	+ 7·8	+18·3	+ 6·3	+ 9·6	+ 0·6	+ 8·0	+ 1·8	−10·4	− 1·7	− 7·4	−14·0	− 8·6	− 3·2	+ 6·8	+19·1	
	+ 6·9	+ 8·2	+ 9·1	+ 6·1	+ 5·6	+ 0·9	+ 0·8	− 4·4	− 9·3	− 8·0	− 5·6	− 2·7	+ 1·3	+ 0·2	+ 5·9	+13·0	
	3h 30m.									3h 30m.						
1833.	+ 5·4	− 0·2	+ 6·9	+ 6·2	+ 5·6	+ 2·2	− 7·4	− 4·2		− 3·7	− 6·4	− 5·2	− 1·9	+ 6·3	+ 8·4	+14·5	
1834.	+ 7·7	+ 9·8	+ 6·3	− 0·2	− 3·8	+ 3·5	+ 3·0	− 3·8	−13·4	− 5·5	− 7·0	− 8·6	− 3·7	− 5·8	+12·8	+22·2	
1835.	+ 6·3	+ 7·0	+12·4	+11·7	+ 2·2	− 0·8	+ 5·0	− 0·9	−11·4	− 8·6	− 2·5	+ 4·0	+ 4·0	+10·2	+ 3·7	+ 5·0	
1836.	+14·5	+12·8	+ 6·2	+ 7·4	− 1·2	+ 2·0	+ 0·1	− 3·4	− 8·2	−11·4	− 2·0	+ 3·7	+12·1	+ 8·8	+ 2·6	+ 5·7	
1837.	+ 0·3	+ 2·8	+ 6·8	+ 9·0	+ 5·0	+ 6·0	− 2·8	− 5·9	− 3·6	− 6·7	− 6·4	+ 0·3	− 5·7	− 1·0	+10·7	+21·3	
1838.	+17·3	+13·7	+ 8·0	+ 5·5	+ 6·2	+ 4·7	+ 7·0	− 4·3	−11·3	− 0·1	− 8·7	− 8·4	−13·4	− 3·0	+11·7	+26·8	
	+ 8·6	+ 7·6	+ 7·8	+ 6·6	+ 2·3	+ 2·9	+ 0·7	− 3·8	− 9·6	− 6·0	− 5·5	− 2·4	− 1·4	+ 2·6	+ 8·3	+15·9	

TABLE (Continued).

	Declination. 4h 30m.										Parallax. 4h 30m.							
	0° to 3°	3° to 6°	6° to 9°	9° to 12°	12° to 15°	15° to 18°	18° to 21°	21° to 24°	24° to 28°		54¼	55¼	56¼	57¼	58¼	59¼	60¼	61¼
	in.	in.	in.	in.	in.	in.	in.	in.	in.		in.	in.	in.	in.	in.	in.		
1833.	+ 6·2	+ 7·2	+ 5·2	+ 1·3	+ 1·8	+ 0·6	− 0·7	− 8·3			− 4·9	− 7·8	− 6·3	+ 3·3	+ 3·3	+14·0		
1834.	+ 0·2	+ 6·8	+ 7·0	+ 7·0	+ 3·6	+ 5·5	− 1·5	− 6·0	−15·0		− 6·4	− 4·2	− 6·3	− 6·1	+ 3·8	+14·5		
1835.	+14·0	+ 9·5	+ 2·0	+ 4·1	+ 8·0	+ 6·8	+ 0·2	− 7·9	−10·1		− 7·7	− 2·9	− 0·3	+ 7·6	+10·5	+ 2·4		
1836.	+10·3	+ 8·4	+ 7·2	+ 7·8	+ 6·5	+ 0·8	− 1·2	− 7·3	− 7·4		−13·5	− 1·6	+ 8·5	+ 8·0	+ 9·1	+ 3·1		
1837.	+10·5	+ 8·8	+ 0·5	+ 2·6	− 0·6	− 3·0	− 1·1	+ 5·7	− 1·4		− 7·0	− 2·0	− 6·0	− 1·2	+ 5·5	+14·7		
1838.	+10·0	+17·7	+10·4	+13·5	+ 7·2	+ 0·6	− 1·7	− 7·8	− 9·0		+ 4·2	− 7·5	−10·0	− 9·5	+ 3·8	+16·0		
	+ 8·5	+ 9·7	+ 5·4	+ 6·0	+ 4·4	+ 1·9	− 1·0	− 5·3	− 8·6		− 5·9	− 4·3	− 3·4	+ 0·4	+ 6·0	+10·8		
	5h 30m.										5h 30m.							
1833.	+ 6·8	+11·0	+ 4·2	− 3·5	− 1·7	+ 2·2	− 2·4	− 4·6			− 4·9	− 8·1	− 4·4	− 0·6	+ 8·2	+13·3		
1834.	+ 7·4	+ 4·8	+ 5·6	+ 7·5	+ 3·5	− 0·9	− 4·3	− 5·5	− 7·7		− 6·3	− 6·7	− 5·6	− 2·0	+ 8·9	+17·7		
1835.	+ 8·0	+11·4	+15·7	+ 5·6	+ 4·5	− 0·6	− 0·7	− 5·6	− 9·1		− 7·6	− 7·5	+ 5·7	+ 0·3	+ 2·1	+10·9		
1836.	+12·3	+18·5	+10·0	+ 8·8	+ 8·7	+ 6·0	− 4·6	− 6·9	− 9·1		−14·3	+ 4·7	+10·7	+14·8	+13·3	+ 0·6		
1837.	+10·0	+ 9·0	+ 9·4	+ 0·3	+ 5·0	+ 7·6	− 1·0	− 8·7	− 6·1		− 6·3	− 4·8	− 3·0	+ 2·9	+ 3·3	+14·7		
1838.	+11·8	+14·8	+ 8·0	+ 4·6	+ 7·8	+ 5·8	+ 2·4	− 6·8	−10·7		+ 1·1	− 8·8	−11·9	− 7·4	+ 9·7	+16·0		
	+ 9·4	+11·6	+ 8·8	+ 3·9	+ 4·6	+ 3·4	− 8·4	− 6·4	− 8·5		− 6·4	− 5·2	− 1·4	+ 1·3	+ 7·6	+12·2		
	6h 30m.										6h 30m.							
1833.	+ 4·0	+ 1·4	+ 9·0	+ 9·0	− 1·8	− 2·9	− 2·7	− 5·5			− 7·4	− 7·3	+ 0·1	+ 6·0	+ 4·1	+13·9		
1834.	+ 8·4	+13·4	+ 6·2	− 2·2	− 0·2	+ 1·0	− 4·1	− 2·4	− 9·5		− 2·3	− 8·4	− 7·7	− 1·9	+ 8·2	+16·3		
1835.	+ 7·8	+ 6·2	+ 3·0	+ 3·7	+ 3·7	+ 2·0	− 4·0	− 3·0	− 9·0		− 8·1	− 3·6	− 4·1	+ 5·7	+ 3·6	+13·7		
1836.	+10·5	+ 7·8	+ 5·5	+ 4·7	− 0·2	+ 2·8	+ 4·0	− 0·9	− 9·1		−14·7	+ 1·4	+ 7·8	+10·0	+ 8·3	+ 5·8		
1837.	+ 5·0	+10·3	+14·2	+10·7	+ 6·0	− 1·6	− 3·0	+ 0·4	− 8·9		− 8·2	− 6·7	+ 2·7	+ 6·2	+ 6·8	+ 8·1		
1838.	+10·6	+ 2·0	+ 8·0	+ 5·2	+ 3·4	+ 2·5	− 1·1	+ 0·1	− 6·5		− 1·1	− 7·1	− 6·8	− 3·7	+ 9·1	+10·9		
	+ 7·7	+ 6·9	+ 7·6	+ 5·2	+ 1·8	+ 0·6	− 1·8	− 1·9	− 8·6		− 7·0	− 5·3	− 1·3	+ 3·7	+ 6·7	+11·5		
	7h 30m.										7h 30m.							
1833.	+ 1·0	+ 7·2	+ 4·5	+ 1·7	+ 4·9	+ 4·2	− 4·3	− 6·0			−10·5	− 5·6	+ 1·7	+ 4·8	+ 5·3	+11·6		
1834.	+ 6·2	+ 5·3	+ 1·0	+ 7·0	− 3·3	− 2·1	+ 1·3	− 3·9	− 7·0		− 5·1	− 8·9	−11·0	− 5·8	+ 5·1	+14·9		
1835.	+12·3	+ 1·5	+ 1·2	− 2·8	+ 1·8	+ 3·3	+ 1·3	− 1·0	− 4·5		− 8·7	− 7·5	− 2·4	+ 2·6	+ 9·0	+14·7		
1836.	+ 2·5	+ 3·3	+ 8·5	+ 9·0	+ 3·6	+ 4·2	− 6·4	+ 1·0	− 6·1		−13·1	− 3·7	+ 0·6	+13·1	+ 8·6	+ 4·3		
1837.	+ 5·7	+ 9·7	+ 0·8	+ 1·0	+ 7·4	+ 8·5	− 3·3	− 5·4	− 3·8		− 8·1	− 7·8	− 0·3	− 0·3	+ 8·3	+ 9·0		
1838.	− 8·3	+ 1·2	+ 4·5	+ 4·0	+ 5·2	+ 4·5	+ 4·2	+ 0·5	− 5·0		− 1·0	− 7·0	− 7·6	+ 2·7	+ 1·3	+ 7·1		
	+ 3·2	+ 4·7	+ 3·4	+ 3·3	+ 2·3	+ 3·8	− 1·2	− 2·5	− 5·3		− 7·8	− 6·8	− 3·2	+ 2·9	+ 6·3	+10·3		
	8h 30m.										8h 30m.							
1833.	+ 8·4	− 0·6	− 2·0	+ 0·3	− 0·3	+ 2·1	− 0·9	− 3·4			− 9·9	− 6·1	− 1·3	− 1·7	+11·7	+ 8·6	+17·0	
1834.	+11·0	+ 1·9	+ 3·8	+ 5·0	+ 6·6	+ 4·3	− 3·0	− 3·4	− 9·3		− 3·7	− 9·3	−12·3	− 9·0	+ 1·2	+13·9	+19·5	
1835.	+ 5·2	+ 3·3	+12·5	+ 5·2	− 2·2	+ 1·0	− 0·7	+ 0·9	− 5·7		−11·0	− 5·3	− 2·5	− 0·1	+ 9·6	+13·0	+15·0	
1836.	+ 9·0	+10·8	+ 5·2	+ 3·0	+ 5·5	+ 2·7	− 1·9	− 5·4	− 4·5		−12·3	− 5·8	+ 7·3	+ 9·4	+ 9·0	+ 6·6	+ 9·8	
1837.	+ 4·8	+11·7	+ 9·7	+ 4·0	+ 4·0	− 3·5	+ 5·6	+ 0·7	− 6·0		−10·7	0	− 1·8	+ 7·0	+ 6·8	+ 9·2	+10·6	
1838.	+ 3·0	− 1·7	0	+ 3·0	+ 1·8	+ 2·0	+ 4·8	+ 6·5	− 4·3		− 2·6	− 7·4	− 1·4	− 6·6	+ 2·8	+ 5·8	+13·3	
	+ 6·9	+ 4·3	+ 4·9	+ 3·4	+ 2·6	0	+ 0·7	− 0·7	− 6·0		− 8·4	− 5·6	− 2·0	− 0·2	+ 6·9	− 9·5	+14·2	
	9h 30m.										9h 30m.							
1833.	+ 9·0	+ 3·8	− 3·0	+ 5·0	+ 0·3	− 1·8	− 0·5	− 3·9			− 9·6	− 4·8	− 6·7	+ 1·7	+ 4·3	+ 9·5	+12·9	
1834.	+ 5·8	+15·2	+15·5	+ 1·5	− 0·6	− 1·0	− 3·9	− 6·1	− 9·5		− 1·3	−11·7	−11·2	−11·1	− 4·2	+ 4·6	+20·8	
1835.	+ 4·8	+ 3·8	− 0·5	− 0·4	+ 7·6	− 0·7	+ 0·9	− 3·3	−11·2		−12·2	− 8·0	− 2·6	− 1·7	+ 6·4	+10·2	+14·2	
1836.	+ 8·5	+10·6	+ 9·7	+ 7·3	+ 2·4	− 0·9	− 0·4	− 3·7	− 6·8		−12·9	− 6·4	− 0·3	+11·6	+10·0	+ 9·7	+ 9·2	
1837.	+11·3	+ 4·6	+ 2·7	+ 7·0	+13·2	+ 6·7	− 5·7	− 1·6	− 7·1		−12·7	− 2·6	+ 5·7	− 2·6	+ 2·0	+11·7	+10·5	
1838.	+10·2	+10·4	+ 3·0	+ 5·4	+ 0·9	+ 1·7	− 2·5	− 1·3	− 6·5		− 6·3	− 8·0	− 5·5	− 8·0	− 7·5	+ 6·5	+14·5	
	+ 8·3	+ 8·1	+ 4·6	+ 4·3	+ 4·0	+ 0·7	− 2·0	− 3·3	− 8·2		− 9·2	− 6·9	− 3·4	− 1·7	+ 1·8	+ 8·7	+13·7	

Table (Continued).

	Declination. $10^h\ 30^m$.											Parallax. $10^h\ 30^m$.							
	0 to 3	3 to 6	6 to 9	9 to 12	12 to 15	15 to 18	18 to 21	21 to 24	24 to 28			$54\frac{1}{2}$	$55\frac{1}{2}$	$56\frac{1}{2}$	$57\frac{1}{2}$	$58\frac{1}{2}$	$59\frac{1}{2}$	$60\frac{1}{2}$	$61\frac{1}{2}$
	in.	in.	in.	in.	in.	in.	in.	in.	in.			in.	in.	in.	in.	in.	in.	in.	in.
1833.	− 1·7	+ 7·0	+ 5·8	− 1·7	− 5·8	+ 5·5	− 3·5	− 1·6		−12·7		− 9·2	− 7·6	− 5·7	− 4·2	+ 2·7	+ 8·0	+13·5	+20·7
1834.	+13·0	+ 4·3	+ 0·8	+ 8·2	+ 8·7	− 2·9	− 0·7	− 6·1		− 4·3		− 4·5	− 5·0	−12·7	−14·5	− 5·0	− 0·1	+16·4	+27·0
1835.	+ 2·6	+ 8·2	+ 7·8	+ 7·0	+ 1·7	− 3·8	+ 0·4	− 5·0		− 4·3		−12·9	− 7·3	− 8·0	+ 3·4	+ 5·3	+11·2	+14·1	+15·0
1836.	+10·3	+ 8·6	+ 5·5	+ 4·5	− 0·7	+ 3·2	− 1·5	− 7·7		− 4·8		−13·1	− 5·8	+ 2·7	+ 3·3	+10·6	+12·8	+ 7·0	+ 9·0
1837.	− 1·0	+ 9·5	+13·8	+ 8·4	− 1·2	+ 5·1	+ 2·0	−12·3		− 6·3		−13·3	− 3·8	− 0·4	+ 3·4	+ 3·3	+ 8·2	+14·1	+13·3
1838.	+ 5·5	+10·0	+10·2	+14·0	+ 1·8	+ 1·7	− 3·6	− 5·6		− 6·0		− 7·2	− 5·4	− 6·0	− 8·0	− 2·6	+ 0·6	+11·9	+28·7
	− 4·8	+ 7·9	+ 7·3	+ 6·7	+ 0·8	+ 1·5	− 1·1	− 6·4		− 6·8		−10·0	− 5·8	− 5·0	− 2·8	+ 2·7	+ 6·8	+12·8	+19·0
	$11^h\ 30^m$.											$11^h\ 30^m$.							
1833.	+ 9·0	− 0·6	+ 1·7	+ 5·5	+ 9·4	− 4·3	+ 0·2	− 5·8				− 8·2	− 6·8	−12·0	− 1·2	− 0·2	+ 2·7	+10·7	+16·9
1834.	+ 3·3	+10·4	+15·7	+ 2·8	− 0·9	+ 8·1	− 4·0	− 9·4		−13·3		− 6·0	−12·4	−19·3	−13·5	− 5·0	− 0·2	+11·4	+21·1
1835.	+ 6·3	+ 8·7	+ 7·7	− 1·2	+ 1·7	+ 3·0	+ 0·6	− 3·9		− 6·1		−12·3	− 1·7	− 8·7	+ 2·1	+ 8·2	+ 8·1	+18·3	+ 7·3
1836.	+ 7·0	+ 8·7	+ 8·0	+ 9·5	+ 8·2	− 2·0	− 2·6	− 4·8		− 7·6		−13·2	− 5·1	− 0·5	+ 5·4	+16·2	+10·9	+ 6·4	+ 5·3
1837.	+16·8	+16·0	− 1·4	+ 9·2	+ 6·3	− 6·2	+ 1·2	+ 5·2		− 9·9		−14·7	− 3·2	− 5·0	+ 1·6	+ 5·0	+10·3	+13·2	+13·5
1838.	+21·3	+14·0	− 0·7	+ 6·2	+ 5·2	+ 2·2	− 5·5	− 8·6		− 4·8		− 6·3	− 5·6	− 8·5	− 9·5	− 0·8	− 9·0	+11·7	+22·1
	+10·6	+ 9·5	+ 5·2	+ 5·3	+ 5·0	+ 0·1	− 1·7	− 4·6		− 8·3		−10·1	− 5·8	− 9·0	− 2·5	+ 3·9	+ 3·8	+12·0	+14·2

Declination. Mean for each Hour of Transit.

1833.												
h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
14 5	14 5	14 37	14 4	14 44	14 42	13 49	13 1	14 15	14 8	14 17	15 1
14 10	13 20	14 12	14 19	14 36	15 15	12 25	14 26	13 16	14 34	14 21	14 23
14 36	14 18	14 9	15 18	13 26	15 29	13 56	14 51	13 44	14 12	12 59	14 54
14 15	14 17	14 54	14 18	14 57	12 37	15 44	14 0	14 53	13 12	14 23	13 52
14 17	14 0	14 28	14 30	14 26	14 31	13 59	14 5	14 2	14 2	14 0	14 32
1834.											
15 58	14 10	16 5	14 22	16 13	14 47	16 10	16 14	14 23	15 56	14 37	16 1
14 46	16 7	14 44	15 57	14 39	16 2	16 6	14 58	15 32	14 22	15 55	13 58
13 54	16 36	14 26	16 42	14 15	15 27	14 20	15 2	14 32	16 29	16 13	14 35
15 37	15 11	16 20	15 1	15 15	15 16	14 12	14 6	16 5	16 3	15 24	14 8
15 4	15 31	15 24	15 31	15 6	15 23	15 12	15 5	15 8	15 43	15 32	14 41
1835.											
17 1	16 22	15 56	15 42	16 29	15 45	15 22	16 42	15 38	16 29	14 59	16 56
15 55	17 59	15 26	17 12	15 33	16 17	15 30	15 22	16 49	15 13	16 34	14 59
15 36	16 22	15 40	17 19	15 43	18 13	16 36	15 39	17 32	14 40	16 45	16 13
17 26	15 26	16 36	15 43	17 26	16 42	16 13	16 46	16 3	17 2	14 30	17 37
16 30	16 32	15 55	16 29	16 18	16 44	15 55	16 7	16 31	15 51	15 42	16 26

TABLE (Continued).

1836.											
h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.
17 27	15 33	18 1	16 15	18 26	16 54	18 41	16 32	17 9	18 4	15 55	17 50
15 40	18 45	15 23	18 59	15 46	18 31	16 14	18 6	15 51	18 4	17 9	17 45
17 55	17 39	17 5	17 53	16 11	18 18	16 41	17 10	17 15	16 14	18 32	15 24
17 56	16 20	18 35	16 24	17 27	17 39	16 59	16 40	18 4	16 40	16 27	17 16
17 15	17 4	17 16	17 23	16 58	17 50	17 9	17 7	17 5	17 16	17 1	17 4
1837.											
18 11	17 28	16 49	18 13	18 17	18 16	19 7	17 18	19 38	16 12	17 17	16 27
18 21	16 52	17 25	16 37	18 39	18 44	18 33	18 32	17 11	18 1	16 36	18 36
16 59	17 2	18 3	16 47	19 19	18 14	15 30	18 26	17 2	17 18	18 1	16 56
15 22	17 19	16 32	18 39	18 28	16 45	17 55	17 16	18 11	17 21	17 54	16 45
17 9	17 10	17 12	17 34	18 41	18 0	17 46	17 58	18 1	17 13	17 27	17 11
1838.											
17 55	18 31	17 14	18 7	17 40	16 3	18 15	17 8	19 5	16 56	19 20	18 22
16 52	18 0	17 20	18 13	17 23	17 18	16 58	18 23	17 34	18 32	18 2	18 22
17 50	17 11	16 28	18 23	17 27	19 6	17 9	18 37	17 14	17 48	17 32	17 51
17 15	18 14	17 36	18 22	18 38	18 0	18 52	17 9	18 19	17 1	18 20	18 8
17 28	17 59	17 10	18 16	17 47	17 37	17 49	17 49	18 3	17 34	18 19	18 11

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XII. *Researches on the Tides.—Eleventh Series. On certain Tide Observations made in the Indian Seas. By the Rev.* W. Whewell, *B.D. F.R.S., Fellow of Trinity College, Cambridge.*

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CERTAIN series of tide observations, made at several places in the Indian Seas, having been forwarded to the Admiralty by the Honourable East India Company, I examined these by the assistance of Mr. D. Ross of the Hydrographer's Office. The observations were very incomplete, as the following account of them will show. But as the tides of those seas offer some very curious phenomena, I endeavoured to discover how far these phenomena were illustrated by the observations thus sent; and I now lay the results of this examination before the Society, in order that they may be preserved, and combined with any information obtained hereafter from these seas. The places of observation were

Coringa Bay, on the coast of Golconda . .	Lat. N.	16°	49′	Long. E.	82°	6′
Cochin, on the Malabar coast		9	57½		76	29
Surat Roads, in the Gulf of Cambay . . .		21	11		73	5½
Gogah, on the opposite side of the Gulf of Cambay		21	41		72	23
Bassadore, at the western extremity of the Island of Kismis, at the entrance of the Persian Gulf		26	39		55	32

1. *At Cochin.*—Although there are two years' tides (1836, 1837) for this place, still they are only taken once in twenty-four hours; and on examination of the heights they seldom vary more than one foot from spring to neap, but the range is only three feet.

2. *Coringa River.*—The observations are also for two years, 1836 and 1837, at this place, but only once in twenty-four hours. These tides appear to be more carefully taken both in times and heights than at Cochin.

3. *In Surat Roads.*—The tides were taken on board an Indian brig, and then only when in harbour, commencing October 18, 1834, and continued to the 23rd. A gap takes place as the brig goes on a cruize till the 31st. Tides continued to Nov. 18; gap to 21st. Tides to December 20; gap to January 7, 1835. Tides to March 29; gap to April 5. Observations end 25th. These tides are for A.M. and P.M.

4. *At Gogah.*—These observations seem to be the most regular; they are taken

at A.M. and P.M.; but there are not four months of them, as they commence on the 10th of June, and end September 30, 1835.

5. *At Bassadore.*—The tides at this place are taken for a few days in September; a few days in October, and the whole of November for 1834.

In the first place, I proceeded to find the establishment of each place, and the semi-menstrual inequality so far as the observations allowed. The following are the results.

	Lunitidal Interval.		Mean, or correct establishment.	Difference.
	Least.	Greatest.		
	h m	h m	h m	m
Coringa Bay ..	9 25	10 39 =	10 2	74
Cochin	0 32	2 21 =	1 26½	109
Surat	3 49	4 59 =	4 24	70
Gogah	4 6	5 27 =	4 46½	81

In the second place, it appeared that at the two places in the Gulf of Cambay, there is an enormous diurnal inequality of the heights, amounting at both Surat and Gogah to not less than seven or eight feet. The observations being laid down in curves, this feature was more marked than in any case which I have yet examined, even than Singapore, of which the curve was given in the Philosophical Transactions for 1837.

The observations give this inequality at Surat and Gogah somewhat irregularly, but not so much so as to prevent my obtaining its epoch in an approximate manner. It appears that the diurnal inequality disappears, and changes its sign, about two days after the moon's declination vanishes.

In the third place, it appeared that at Bassadore there is a very large diurnal inequality of the *times*, amounting to above two hours in some instances. This is a new case: for though I had already ascertained that in some places there is a diurnal inequality of the lunitidal intervals, I had never supposed that it could amount to a quantity so large as this, which indeed utterly displaces the tides. For instance, about the 23rd of November 1834, the tides on the afternoon of each day were earlier by about two hours than the hour of the tides in the forenoon. What makes this anomaly still more remarkable is, that at this place there is little or no diurnal inequality of the heights. As this result is a very novel one, I shall here give a copy of the original observations.

Bassadore Original Tide Register for the Month of November 1834.

		Low Water.		High Water.		Range.			Low Water.		High Water.		Range.
		Time.	Height.	Time.	Height.				Time.	Height.	Time.	Height.	
		h m	ft. in.	h m	ft. in.	ft. in.			h m	ft. in.	h m	ft. in.	ft. in.
Nov. 1	A.M.	5 35	1 6	11 58	9 0	7 6	Nov. 16	A.M.	5 55	3 0	11 55	9 0	6 0
	P.M.	6 32	0 0	....	....	....		P.M.	6 43	0 6	13 12	8 6	8 0
2	A.M.	6 15	1 0	0 26	9 6	8 6	17	A.M.	6 17	2 0	11 34	8 6	6 6
	P.M.	7 4	0 0	0 30	9 6	9 6		P.M.	6 35	1 6	13 22	9 0	7 6
3	A.M.	7 1	1 0	1 23	9 6	8 6	18	A.M.	4 46	2 0	....	....	....
	P.M.	8 3	1 0	1 2	9 0	8 0		P.M.	7 44	3 0	37	9 0	6 0
4	A.M.	7 39	1 0	2 14	9 0	8 0	19	A.M.	7 24	3 0	2 12	8 6	5 6
	P.M.	8 53	2 0	1 32	9 6	7 6		P.M.	8 20	1 6	1 6	8 6	7 0
5	A.M.	8 28	2 0	3 26	9 0	7 0	20	A.M.	7 57	2 0	2 55	9 0	7 0
	P.M.	8 33	2 0	2 17	9 0	7 0		P.M.	9 7	2 0	1 35	9 0	7 0
6	A.M.	9 13	2 6	4 4	9 0	6 6	21	A.M.	8 48	3 0	3 57	8 6	5 6
	P.M.	10 16	2 6	2 58	9 0	6 6		P.M.	9 44	2 6	2 8	9 0	6 6
7	A.M.	10 10	2 6	5 0	8 6	6 0	22	A.M.	9 16	2 0	4 34	8 6	6 6
	P.M.	11 14	4 0	3 38	8 0	4 0		P.M.	10 30	3 0	2 38	8 6	5 6
8	A.M.	11 35	4 0	6 15	7 6	3 6	23	A.M.	13 13	4 0	5 33	8 6	4 6
	P.M.	....	....	5 3	7 6			P.M.	11 39	4 0	3 41	8 0	4 0
9	A.M.	26	2 6	7 35	7 0	4 6	24	A.M.	11 27	4 0	6 43	8 0	4 0
	P.M.	1 18	4 0	7 7	7 6	3 6		P.M.	....	....	5 7	8 6	
10	A.M.	2 48	3 0	8 41	7 0	4 0	25	A.M.	0 52	4 0	7 50	8 0	4 0
	P.M.	2 9	3 0	7 27	7 0	4 0		P.M.	1 4	4 0	6 29	9 0	5 0
11	A.M.	2 23	3 0	9 40	8 0	5 0	26	A.M.	1 52	4 0	8 48	8 6	4 6
	P.M.	4 3	3 0	9 41	8 0	5 0		P.M.	2 32	3 0	8 8	9 6	6 6
12	A.M.	3 50	3 0	10 30	8 0	5 0	27	A.M.	2 30	3 0	9 12	9 0	6 0
	P.M.	4 58	3 0	10 25	8 0	5 0		P.M.	2 31	2 0	9 38	9 0	7 0
13	A.M.	5 4	3 0	10 42	9 0	6 0	28	A.M.	3 39	2 0	9 37	9 0	7 0
	P.M.	6 25	3 0	11 7	9 0	6 0		P.M.	4 20	1 0	10 42	9 0	7 0
14	A.M.	4 23	4 0	10 44	9 0	5 0	29	A.M.	4 12	1 0	10 21	9 6	8 6
	P.M.	5 43	3 0	11 54	9 0	6 0		P.M.	5 18	1 0	11 49	10 0	9 0
15	A.M.	5 15	3 0	11 26	9 0	6 0	30	A.M.	5 20	2 0	11 15	9 9	7 9
	P.M.	6 8	3 0	12 29	9 0	6 0		P.M.	6 9	0 0	12 22	9 9	9 9

To this table there is no signature, but all the others for Bassadore are signed Thomas Elroon, Commodore.

In order to bring into view the diurnal inequality of the times in these observations, we take out the times of moon's transit, corrected for the longitude of Bassadore. We hence find the interval between each transit and the succeeding time of high water, as will be seen below; and it appears that the *lunitidal intervals* vary from $9^h\ 48^m$ to $13^h\ 33^m$. At the beginning of the month the successive lunitidal intervals are equal. About the 7th they become alternately about 12^h and 10^h, or 13^h and 11^h; on the 12th they are again equal; about the 19th they again become alternately about 12^h and 10^h. The difference gradually diminishes to the 20th, when they are again equal; after which the inequality reappears, and continues to the end of the month.

This diurnal inequality of above two hours in the time of high water, in a situation in which the diurnal inequality of height is insensible, I cannot but consider as a most curious tidal feature in addition to those already remarked in the Indian Seas.

	Moon's Transit.	Succeeding Tide.	Lunitidal Interval.		Moon's Transit.	Succeeding Tide.	Lunitidal Interval.
	h m	h m	h m		h m	h m	h m
1834. 31	23 48	11 58	12 10	Nov. 16	12 5	13 12	13 7
Nov. 1					12 28	11 34	11 6
	16	26	12 10	17	12 52	13 22	12 30
2	45	30	11 45		13 17	37	11 20
	1 14	1 23	12 9	18	13 42	2 12	12 30
3	1 44	1 2	11 18		14 8	1 6	10 58
	2 14	2 14	12 0	19	14 35	2 55	12 20
4	2 44	1 32	10 48		15 2	1 35	10 33
	3 14	3 26	12 12	20	15 29	3 57	12 28
5	3 43	2 17	10 34		15 56	2 8	10 12
	4 12	4 4	11 52	21	16 23	4 34	12 11
6	4 41	2 58	10 17		16 50	2 38	9 48
	5 8	5 0	11 52	22	17 17	5 33	12 16
7	5 35	3 38	10 3		17 43	3 41	9 58
	6 1	6 15	12 14	23	18 8	6 43	12 35
8	6 26	5 3	10 37		18 34	5 7	10 33
	6 49	7 35	12 46	24	18 59	7 50	12 51
9	7 12	7 7	11 55		19 24	6 29	11 5
	7 34	8 41	13 7	25	19 49	8 48	12 59
10	7 56	7 27	11 31		20 14	8 8	11 54
	8 17	9 40	13 23	26	20 40	9 12	12 32
11	8 37	9 41	13 4		21 6	9 38	12 32
	8 57	10 30	13 33	27	21 32	9 37	12 5
12	9 17	10 25	13 8		21 59	10 42	12 43
	9 37	10 42	13 5	28	22 26	10 21	11 55
13	9 57	11 7	13 10		22 54	11 49	12 55
	10 17	10 44	12 27	29	23 23	11 15	11 52
14	10 38	11 54	13 16		23 53	12 22	12 29
	10 59	11 26	12 27	30			
15	11 20	12 29	13 9				
	11 42	11 55	12 13				

XIII. *Account of Experiments on Iron-built Ships, instituted for the purpose of discovering a correction for the deviation of the Compass produced by the Iron of the Ships.* By George Biddell Airy, *Esq. A.M., Astronomer Royal.*

Received April 11,—Read April 25, 1839.

Section I.—*Preliminary.*

In the months of October and November 1835, a series of observations was made under the direction of the Board of Admiralty, by Commander Johnson, R.N., on the iron steam-ship Garry Owen, for ascertaining the amount of disturbance of the compass produced by the magnetic attraction of the iron, of which the ship's sides and bottom are composed. The details of these experiments are published in the Philosophical Transactions for 1836. Results of great importance were obtained as to the amount of deviation of the compass in different parts of the ship; and several remarkable experiments were described, which seemed to prove that the ship acted upon external compasses, in the manner of a permanent magnet. But no attempt was made to discover the laws of the magnetic disturbance, or to ascertain its causes; and no attempt could therefore be made to neutralize the ship's disturbing force by the introduction of new disturbing forces.

The last-mentioned point was, however, kept in sight by the Board of Admiralty, and partial arrangements were made for conducting a series of experiments referring expressly to this subject, whenever a favourable opportunity should occur. In the month of July 1838 the iron-built steam-ship the Rainbow was placed by the General Steam Navigation Company at the service of the Admiralty for magnetic examination. The conduct of the experiments was entrusted by the Board to me, and the vessel was immediately placed in the Basin of the Deptford Dock Yard.

The first point to be settled was the selection of stations in which the deviations of the compass should be observed. The object which I proposed to myself (the ascertaining the laws of the deviation, and the neutralization, if possible, of the deviating forces) made it a matter of no interest to me to try the action of the compass in many different places. I determined therefore on selecting only those in which it was likely that the compass, in the ordinary course of navigation, might be used for steering. With the assistance of Capt. Shirreff, R.N., Captain Superintendant of the Deptford Dock Yard, I fixed on the four following stations:

Station I., very near the binnacle as fixed in the ship, at the distance of 13 feet 2 inches from the extreme part of the stern.

Station II., at the place where the binnacle would probably be fixed in a steam-ship of the Royal Navy, at the distance of 31 feet 9 inches from the stern.

Station III., as near to the mizen-mast as observations could conveniently be made, distant 48 feet 3 inches from the stern.

Station IV., a short distance abaft the fore-mast, at the distance of 47 feet from the knight-head, or 151 feet 6 inches from the stern.

For the purpose of commanding a clear view above the engine-boxes and paddle-boxes, it was necessary to place in each of these positions a stage of considerable height (the elevation of its upper floor was 10 feet 2 inches). A lower floor was fixed in the stage at the elevation of 3 feet 6 inches above the deck; and upon this the azimuth compass used in the experiment was always placed. The elevation of its card above the deck was then about 4 feet ½ inch. This elevation was adopted as coinciding almost exactly with that at which the compass was mounted in the binnacle when the ship was placed under my management.

The trouble of observing a compass upon the top of the stage was so small, that (although unimportant for the special objects of this inquiry) it was thought desirable to ascertain its error, as well as that of the lower compass, in each position of the vessel's head. In the observations of the first day the upper compass was placed upon the stage, with its card raised 10 feet 8 inches above the deck; it was afterwards raised 2 feet higher.

In adopting a method of observation, the following considerations were taken into account.

If we were certain that no sensible effect would be produced on the compass by the iron in the wharfs and the various buildings surrounding the basin, the easiest method of determining the error of the ship's compass would be to compare it with a compass on shore, either by reciprocal observations, or by observing with both the line of the ship's fore-mast and mizen-mast, or by observing with the ship's compass a mark at some distance, and observing with the shore compass the azimuth of the same mark when seen in the same line with the ship's compass. I omit the mention of a mark at so great a distance that its parallax in the movement of the ship would be insensible, because no very distant mark can be seen from the Deptford Dock Yard. But these methods require a general certainty that there are no local disturbing causes. The vessel had been placed so early in the basin, that I had had no opportunity of examining into the existence of local disturbances; and, as far as the general aspect of the localities could suggest an opinion, it was extremely probable that the local disturbances would be sensible. There are several iron posts, and an iron crane, very near the basin wall; there was a great mass of iron tanks in a shed adjoining it; and the great length of the ship brought the compass when at Station I. very near to one side or another of the basin. It appeared therefore best to observe the azimuth of a chimney on the opposite side of the river, with the ship's compass only, and to register the local position of the compass in such a manner that the correction depend-

ing upon its place in the basin might be taken from a map; and it was supposed that by afterwards carrying a compass round the basin when the ship was removed, observing with it the same chimney, and correcting for local position in the same manner, the amount of error depending on local disturbance might be found, and applied, if necessary, to the observations in the ship.

The observations were made in the following manner.

Through a hole in the upper floor of the stage a wooden spindle, 2½ inches square, was passed. The lower end of this spindle was connected with a wooden fork, whose branches lodged upon the gimble-frame of the azimuth-compass, in such a manner that when the spindle was turned it carried with it the frame of the compass. At the upper end of the spindle was a small telescope with a thick wire in its field of view; the height of the telescope above the deck was about 15 feet 6 inches; so that a person standing on the upper floor of the stage could with the telescope conveniently observe the chimney above the paddle-boxes and engine-boxes, while a person below could read off the bearing by the azimuth compass in the usual way. At the beginning and at the end of each series, or at other convenient times, the bearing of the chimney was observed as well with the sights of the azimuth compass as with the telescope; and thus was obtained an index error of the telescope, by which all observations made with the telescope could be converted into equivalent observations with the compass sights.

Two theodolites were stationed at points commanding the chimney and every part of the basin. The duty of the theodolite observers was, at each position of the ship, to observe the wooden spindle placed on the compass, to observe the chimney, and to make reciprocal observations.

The person upon the top of the stage, having ascertained that the ship was steadily moored, directed that the ship's bell should be struck, as a signal to all the observers, and then gave out the ordinal number of the observation. This was entered by each of the observers in his book. The person on the stage directed his telescope to the chimney and gave notice thereof to the person below, who read off the compass and entered the reading in his book: then the person on the stage directed his telescope to the centre of the ship's funnel, and the compass in like manner was read by the person below: these observations were repeated; then the person at the top of the stage observed the chimney with the upper compass, and entered the reading in his book. During these observations, each of the theodolite-observers had twice observed the compass-spindle, the chimney, and the other theodolite, and entered the readings in his book; and other observations were made and entered by other persons, as will shortly be mentioned. Each observer gave signal of the completion of his observations by raising a flag: as soon as all had finished, the word was given for swinging the ship into a new position.

On the first two days of observation, July 26 and 30, a compass was placed upon one of the paddle-boxes, and the azimuth of the chimney was regularly observed with

it. The disturbances of the compass, however, appeared anomalous; and as it did not appear probable that this compass could be corrected so completely as the others, the observations were never wholly reduced.

In the first two days there were mounted on shore, near the western side of the basin, a dipping needle, and a two foot horizontal needle, suspended by a skein of silk, and carrying a reflector with which the divisions of a scale were observed in GAUSS's method; and near the south side of the basin a dipping needle, and a two foot horizontal needle transverse to the meridian, carried by two parallel skeins of silk in the manner of GAUSS and WEBER's bifilar magnetometer, and carrying a reflector with which a divided scale was observed. These were observed when the signal was given for the compass observations.

On the third day, July 31, three dipping needles were placed in the ship, in positions very near to the compass stations I. III. IV.: during one complete revolution of the ship they were placed so that the vibrations of the needle were in the plane of the keel; and during another complete revolution, they were so placed that the vibrations were transverse to the keel.

The same dipping needles were afterwards, on August 2, carried on shore, and arranged in a line nearly E. and W. for observation of the effect produced by bringing the ship's head and stern very near to the side of the basin while the keel was E. and W. They were then arranged in a N. and S. line, and the effects of the two extremities of the ship while the keel was N. and S. were examined.

The ship was then taken out of the basin, and on August 6 the same compass which had been used at the lower stage-floor in the four stations on board the ship, was mounted upon a stage carried by a raft (in the construction of which there was not much iron) and floated round the basin. The height of the compass above water was made nearly the same as the height of the lower stage floor (in the ship) above water; and the raft was carried to all parts where there was suspicion of local disturbance, care being taken that it should be placed nearly opposite each iron post, and nearly intermediate to adjacent posts.

On August 14 observations of horizontal intensity were made with a needle suspended by a silk fibre, at each of the four stations, the ship's head being placed successively N., W., S., and E.*, by means of a compass on shore.

On August 20 and August 22, after the application of correctors, observations of the compasses were made in the same manner as those made for ascertaining the amount of error, in order to verify the accuracy of the correction.

I have now only to give the names of the persons employed in the various parts of the operation.

The swinging the ship round was managed by Captain SHIRREFF or by Mr. MORRICE

* It is always to be understood that the cardinal points of the compass and the azimuths are referred to the *magnetic* meridian.

(Master Attendant of the Dock Yard), with the assistance of men attached to the Dock Yard.

The two theodolites (A. and B.) were assigned to Mr. JAMES GLAISHER and Mr. ELLIS, Assistants at the Royal Observatory.

The western dipping needle and horizontal needle (C.) were observed, on July 26, by Professor CHRISTIE, and on July 30 by the Rev. R. SHEEPSHANKS.

The southern dipping needle and horizontal needle transverse to the meridian (D.) were observed by the Rev. R. MAIN, First Assistant at the Royal Observatory.

The observations with the compass on the paddle-box (E.) were made by Captain SHIRREFF.

The upper telescope was directed and the upper compass (F.) read off by myself in the whole of the experiments.

The compass on the lower stage-floor (G.) was read off on July 26 by Lieutenant DENISON, R.E., on July 30 by FRANCIS BAILY, Esq., and in all the subsequent observations by Captain SHIRREFF.

The compass on the raft (H.) was observed by Mr. MAIN, with the assistance of Captain SHIRREFF.

Of the three dipping-needles on board, on July 31, the sternmost was read by Captain SHIRREFF, and the other two by Mr. MAIN.

The dipping needles on shore, on August 2, were read by Mr. MAIN.

The observations of the time of vibration for horizontal intensity on August 14 were made by myself, with the assistance of Captain SHIRREFF.

To all the persons named above my most cordial thanks are due for the zeal and steadiness with which they followed out my plans under the most distressing circumstances of weather. But to Captain SHIRREFF in particular an acknowledgement of my obligations must here be given. Not only was the assistance of men and materials from the Dock Yard furnished by Captain SHIRREFF in the way which I thought most desirable, but by his presence and by the interest in the operations which he displayed, the services of all the subordinate persons were rendered fully efficient; while the part which he took as an active observer, from the beginning to the end, materially lightened my labours and increased my confidence in the results.

Section II.—*Immediate Results of Observations.*

I. Local disturbances of the compass, as shown by the observations on the Raft.

A plan of the Basin in Deptford Dock Yard was prepared, from simultaneous observations with the two theodolites A. and B., on a signal carried to different parts of the basin-wall, and was copied (by pricking off) for the insertion of the places of the compass in every series of observations. The positions of the compass when on the raft were laid down on one of these copies, from simultaneous observations with the theodolites, as already described. The positions from No. 105 to 114 range along the E. and S.E. side of the basin, from the entrance to the S. angle: 115, 116, and 117, are on the S.W. side between the ways of two building

slips; and the remainder, from 118 to 129, are on the N.W. side, from the W. angle to the entrance. The distance of the compass from the wharf varied from 16 to 24 feet. At each of these positions the chimney was observed with the compass. The next step (as in all the other observations) was to correct for locality, and this was done in the following manner. The angle at A, the south-eastern theodolite, between the chimney and the theodolite B. was 59° 29′, and the angle at B. between the chimney and A. was 113° 38′. Consequently the angle subtended by A B at the chimney was 6° 53′. The line A B being, therefore, supposed to represent 6° 53′, and being divided into portions corresponding each to 1°, commencing at B, dotted lines were drawn from the chimney through the points forming these divisions, and thus indicated the points in the basin at which the true azimuth of the chimney differs 1°, 2°, 3°, &c., from the azimuth observed at B. If then the correction + 1° be applied to the azimuths (reckoned from north through east, south, and west), as observed with the compass at any point in the dotted line marked + 1°, an azimuth will be obtained which ought to be the same as if it had been observed at B, and whose difference from that at B can be occasioned only by local disturbance; and similarly for positions of the compass upon any other dotted lines. For intermediate positions the correction was taken to 5′ by inspection. In this manner the following Table was formed.

No. of observation.	Observed azimuth reduced to B.	Mean of all the observed azimuths reduced.	Apparent local disturbance.	No. of observation.	Observed azimuth reduced to B.	Mean of all the observed azimuths reduced.	Apparent local disturbance.
	° ′	° ′	° ′		° ′	° ′	° ′
105	37 25	37 46	−0 21	118	38 45	37 46	+0 59
106	37 10		−0 36	119	38 35		+0 49
107	38 5		+0 19	120	38 10		+0 24
108	36 30		−1 16	121	38 10		+0 24
109	37 30		−0 16	122	37 0		−0 46
110	37 45		−0 1	123	38 5		+0 19
111	37 45		−0 1	124	38 0		+0 14
112	37 40		−0 6	125	38 25		+0 39
113	37 45		−0 1	126	37 35		−0 11
114	37 40		−0 6	127	35 55		−1 51
115	38 5		+0 19	128	39 0		+1 14
116	38 15		+0 29	129	37 5		−0 41
117	37 55		+0 9				

The magnitude of these disturbances is so small as to leave it almost doubtful whether any correction ought to be applied for them. There is evidently no need for correction when the place of the compass is far removed from the side of the basin, as it was in all positions of the compass except at Station I. The only instances of the observations at Station I. in which corrections for local disturbance are applied are the following:

Nos. 1, 2, 3, 4, 6, 8, (nearly in the position of 105) + 0° 20′ each.
No. 24 (near the place of 119). . . . . − 0° 50′ each.
No. 25 (between 120 and 121). . . . . − 0° 25′ each.
No. 33 (near 106) . . . . . + 0° 30′ each.

II. Azimuths of the ship's head and disturbance of the compass on the lower stage floor at Station I.

The chimney having been observed with the telescope carried by the spindle mounted on the compass, the reading of the compass card was at first expressed in the azimuths or amplitudes used by nautical men. This was then converted into azimuth reckoned from the N. through the E., S., and W., to N. proceeding from 0° to 360°. Then the correction for index error of the telescope was applied: then the correction for locality, and then (if necessary) the correction for local disturbance. The azimuth thus corrected would have been 37° 46′, had the ship caused no disturbance. The excess of the corrected azimuth above 37° 46′ is set down as the disturbance of the compass by the ship; the sign + denotes that the azimuth appears too great, or that the needle is deflected to the left.

The funnel of the steam-ship having been observed in the same way, its apparent azimuth, as affected by the ship's disturbance of the compass, was found in the same manner (omitting the correction for locality): correcting this for the disturbance (already found), the true azimuth was obtained.

In this manner the following table was formed.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
	° ′	° ′		° ′	° ′
1	203 5	−16 50	19	1 35	+40 45
2	214 53	−22 45	20	19 25	+51 20
3	220 0	−23 20	21	38 25	+50 50
4	230 0	−30 5	22	57 45	+46 30
5	240 50	−34 5	23	76 55	+39 15
6	248 50	−36 40	24	93 10	+33 5
8	256 4	−40 2	25	109 0	+26 40
9	267 44	−44 27	26	125 50	+19 40
10	276 12	−47 50	27	142 10	+11 55
11	282 57	−49 27	28	159 10	+ 4 20
12	292 17	−52 25	29	171 5	− 1 35
13	294 0	−53 45	30	189 45	−11 0
14	300 35	−54 50	31	202 5	−16 45
15	308 0	−53 50	32	220 25	−25 10
16	313 47	−54 2	33	248 55	−37 50
17	321 40	−50 10	34	278 15	−48 25
18	338 25	−33 0			

No. 7. was inadvertently omitted in the numeration. It will be remarked that the general positions of the vessel's head from No. 1 to 11 coincide nearly with those from No. 30 to 34. The following points are worthy of attention; 1st, that between Nos. 18 and 19, upon changing the position of the vessel's head 23°, the disturbance was altered 74°, so that the change of the vessel's position appeared on the compass card to be nearly 97°; 2nd, that the maximum of the positive errors is distinctly less than the maximum of the negative errors. The latter remark will be found to bear in an important degree on the theoretical explanation of the errors.

III. Disturbance of the compass on the lower stage floor at Station II.

The operation is in every respect the same as the last, except that no correction was applied for local disturbance.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
35	282° 51′	−21° 36′	45	103° 36′	+12° 54′
36	300 4	−12 41	46	124 26	+ 7 26
37	320 49	− 1 19	47	143 57	+ 1 26
38	340 7	+ 9 31	48	163 16	− 4 16
39	353 48	+15 4	49	179 6	− 9 6
40	11 26	+18 34	50	203 1	−16 16
41	31 14	+19 46	51	222 21	−21 51
42	49 47	+19 51	52	244 56	−26 26
43	67 3	+18 12	53	267 46	−24 16
44	85 1	+14 59			

IV. Disturbance of the compass on the lower stage-floor at Station II., when the steam was up and the boilers and engine hot.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
90	279° 1′	−21° 31′	98	99° 11′	+15° 19′
91	296 36	−13 6	99	115 26	+11 4
92	315 54	− 4 54	100	143 26	+ 3 4
93	329 6	+ 3 54	101	174 21	− 6 21
94	354 26	+12 34	102	218 26	−20 26
95	14 36	+16 54	103	246 41	−23 11
96	37 41	+19 49	104	275 1	−26 1
97	62 26	+18 34			

If these disturbances and those of the last Table be constructed graphically, the azimuth being taken as abscissa, and the disturbance of the compass as ordinate, and if a curve in each case be drawn through the points, it will be found that the curves do not sensibly differ. It appears therefore that no sensible part of the disturbance depends on the state of heat of the engines.

V. Disturbance of the Compass on the Lower Stage Floor at Station III.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
54	(274° 11′)	− 19° 41′	64	93° 11′	+ 11° 49′
55	(310 24)	− 13 24	65	120 36	+ 7 54
56	311 18	− 4 18	66	139 46	+ 3 44
57	324 26	+ 2 49	67	163 9	− 3 9
58	340 51	+ 7 39	68	187 9	− 10 39
59	2 34	+ 11 56	69	216 6	− 18 21
60	22 16	+ 14 29	70	235 11	− 21 41
61	38 11	+ 16 19	71	259 59	− 24 29
62	60 34	+ 15 26	72	275 41	− 21 41
63	81 36	+ 13 24			

It is conjectured that the azimuth in No. 54 ought to be increased 10°, and that in No. 55 ought to be diminished 10°; and the subsequent calculations are made on this supposition.

VI. Disturbance of the Compass on the Lower Stage Floor at Station IV.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
	° ′	° ′		° ′	° ′
73	276 21	− 9 21	82	95 6	+ 7 9
74	291 6	− 2 6	83	130 6	+ 3 54
75	321 6	+ 0 24	84	146 46	− 0 46
76	330 36	+ 8 54	85	170 16	− 6 16
77	347 6	+ 8 54	86	198 1	− 12 31
78	3 41	+ 9 49	87	219 36	− 15 21
79	16 46	+ 10 14	88	245 9	− 15 39
80	49 54	+ 7 36	89	264 34	− 12 4
81	66 59	+ 8 1			

The azimuth or the disturbance in No. 75 appears to be wrong. It is not used in the subsequent calculations.

(To avoid confusion, I shall defer to a subsequent part of this paper the account of the disturbance of the compass on the upper stage floor.)

VII. Times of Vibration of a Needle suspended by a Silk Fibre near the Level of the Lower Stage Floor.

	s
On shore 30 vibrations were performed in	140·6
At Station I., the ship's head being N., the time of 30 vibrations was .	300·5
the ship's head being E., the time of 30 vibrations was .	119·5
the ship's head being S., the time of 30 vibrations was .	103·6
the ship's head being W., the time of 30 vibrations was .	133·9
At Station II., the ship's head being N., the time of 30 vibrations was .	166·3
the ship's head being E., the time of 30 vibrations was .	129·7
the ship's head being S., the time of 30 vibrations was .	120·8
the ship's head being W., the time of 30 vibrations was .	157·3
At Station III., the ship's head being N., the time of 30 vibrations was .	153·4
the ship's head being E., the time of 30 vibrations was .	130·2
the ship's head being S., the time of 30 vibrations was .	121·7
the ship's head being W., the time of 30 vibrations was .	158·5
At Station IV., the ship's head being N., the time of 30 vibrations was .	147·8
the ship's head being E., the time of 30 vibrations was .	130·7
the ship's head being S., the time of 30 vibrations was .	128·8
the ship's head being W., the time of 30 vibrations was .	172·3

VIII. Observations with Dipping Needles on board the Ship, near the Level of the Stage Floor at Stations I., III., IV., the Needles vibrating in the Vertical Plane passing through the Ship's Keel.

(The dipping-needle employed at Station I. is a small needle of very fine workmanship, by DOLLOND, lent by Mr. DOLLOND for the experiments. Its dip on shore appeared to be 72° 40′.

The dipping-needle used at Station III. is a larger needle, by JONES, lent by Mr. CHRISTIE. Its dip on shore was about 69° 10′.

The dipping-needle employed at Station IV. is an excellent needle belonging to the Admiralty, made by DOLLOND, of intermediate dimensions. Its dip on shore was about 69° 35′.)

The letter H denotes that the dip was towards the head, S that it was towards the stern. When the number of degrees exceeds 90° the needle is dipping on the side of the vertical, opposite to that denoted by a number of degrees less than 90.

No. of observation.	Azimuth of ship's head.	Dip at Station I.	Dip at Station III.	Dip at Station IV.
	° ′	° ′	° ′	° ′
35	282 51	78 50 S.	89 35 H.	89 20 H.
36	300 4	.....	83 21	79 55
37	320 49	87 20 S.	78 0	78 42
38	340 7	89 25 H.	72 6	69 10
39	353 48	88 10	71 37	68 10
40	11 26	88 5 H.	71 55	67 12
41	31 14	89 10 S.	74 47	68 32
42	49 47	86 10	79 45	65 17
43	67 3	82 30	84 59	85 35 H.
44	85 1	78 20	92 18 H.	88 20 S.
45	103 36	72 15	79 3 S.	78 50
46	124 26	68 0	71 53	67 0
47	143 57	64 55	66 58	61 30
48	163 16	61 50	63 28	58 25
49	179 6	61 5	62 28	58 10
50	203 1	61 50	63 55	58 30
51	222 21	64 40	68 40	68 22
52	244 56	68 35	75 10	69 5
53	267 46	74 5 S.	83 43 S.	79 32 S.

IX. Observations with dipping-needles near the level of the stage-floor at Stations I., III., IV.; the needles vibrating in the vertical plane transverse to the ship's keel.

(The needles are the same as those used in the last experiments.) The letter S. denotes that the dip was to the starboard, L. that it was to the larboard.

No. of observation.	Azimuth of ship's head.	Dip at Station I.	Dip at Station III.	Dip at Station IV.
54	284° 11′	78° 30′ S.	74° 45′ S.	70° 2′ S.
55	300 24	79 25	76 16	70 32
56	311 18	83 30	78 15	77 25
57	324 26	85 30	81 30 S.	80 58
58	340 51	89 5 S.	.. ..	87 58 S.
59	2 34	86 0 L.	.. ..	82 15 L.
60	22 16	80 0	79 16 L.	69 38
61	38 11	76 15	73 26	68 57
62	60 34	72 40	68 8	61 40
63	81 36	69 10	66 29	60 45
64	93 11	71 0	66 15	60 42
65	120 36	72 30	68 54	62 25
66	139 46	76 0	73 56	63 40
67	163 9	78 55	79 30	72 52
68	187 9	87 40 L.	89 3 L.	89 50 L.
69	216 6	85 0 S.	82 12 S.	83 45 S.
70	235 11	81 35	77 50	73 25
71	259 59	78 55	74 32	69 40
72	275 41	78 30 S.	74 40 S.	69 40 S.

(The observations with the dipping-needle and large magnets on shore will be deferred to another part of this paper.)

Section III.—*Theory of Induced Magnetism.*

The fundamental supposition of the theory on which I shall found the calculations in the following pages is, that, by the action of terrestrial magnetism, every particle of iron is converted into a magnet whose direction is parallel to that of the dipping-needle, and whose intensity is proportional to the intensity of terrestrial magnetism: the upper end having the property of attracting the north end of the needle, and the lower end that of repelling it.

It would have been desirable to make the calculations on Poisson's theory, which undoubtedly possesses greater claims on our attention, as a theory representing accurately the facts of some very peculiar cases, than any other. The difficulties, however, in the application of that theory to complicated cases, are great, perhaps insuperable. And in ordinary cases, the simpler theory that I have mentioned will give the same comparative though not the same absolute results. For instance, Poisson's theory explains the near equality between the attraction of a sphere or thick shell and that of a thin shell: the simpler theory does not explain this near equality; but

the shell being given, it enables us to compute its comparative effects in different positions as well as Poisson's. In the case of a spheroidal mass of iron, the result is similar. And as far as giving a general agreement between the results of calculation and the comparative effects of given masses in different positions, the theory appears sufficiently accurate.

Let the place of the compass be the origin of co-ordinates: let A be the azimuth of the ship's head, measured from the magnetic north towards the east: a the azimuth of any particle measured from the ship's head; so that $A + a$ is the azimuth of that particle from the north. Let b be the angular depression of the particle. Then if r be the distance of the particle from the compass; x, y, z, the ordinates towards the north, towards the east, and vertically downwards; we shall have

$$x = r \,.\, \cos b \,.\, \cos \overline{A + a}, \qquad y = r \,.\, \cos b \,.\, \sin \overline{A + a}, \qquad z = r \,.\, \sin b.$$

Let I represent the intensity of terrestrial magnetism, δ the dip, m a constant depending on the mass of the particle, $2\,l$ the length of the small magnet into which it is changed. Then the ordinates of that end of the small magnet which attracts the north end of the needle will be

$$x - l\cos\delta, \qquad y, \qquad z \quad l\sin\delta.$$

Its distance (omitting the squares, &c. of l) $= \sqrt{\{r^2 - 2\,l\,x\cos\delta - 2\,l\,z\sin\delta\}}$

$$= r - \frac{l}{r}(x\cos\delta + z\sin\delta).$$

And the resolved parts of its attraction (supposing the whole attraction inversely as the nth power of the distance) will be—In the direction of x,

$$\mathrm{I}\,m \frac{x - l\cos\delta}{\left\{r - \frac{l}{r}(x\cos\delta + z\sin\delta)\right\}^{n+1}}$$

$$= \mathrm{I}\,m \,.\, \frac{x}{r^{n+1}}\left\{1 - l \,.\, \frac{\cos\delta}{x} + l \,.\, \overline{n+1} \,.\, \frac{x\cos\delta + z\sin\delta}{r^2}\right\}.$$

In the direction of y,

$$\mathrm{I}\,m \frac{y}{\left\{r - \frac{l}{r}(x\cos\delta + z\sin\delta)\right\}^{n+1}}$$

$$= \mathrm{I}\,m \frac{y}{r^{n+1}}\left\{1 + l \,.\, \overline{n+1} \,.\, \frac{x\cos\delta + z\sin\delta}{r^2}\right\}.$$

In the direction of z,

$$\mathrm{I}\,m \frac{z - l\sin\delta}{\left\{r - \frac{l}{r}(x\cos\delta + z\sin\delta)\right\}^{n+1}}$$

$$= \mathrm{I}\,m \,.\, \frac{z}{r^{n+1}}\left\{1 - l \,.\, \frac{\sin\delta}{z} + l \,.\, \overline{n+1} \,.\, \frac{x\cos\delta + z\sin\delta}{r^2}\right\}.$$

In like manner, the repulsive force produced by the other end of the small magnet will be—In the direction of x

$$= \mathrm{I}\, m \frac{x}{r^{n+1}} \left\{ 1 + l . \frac{\cos \delta}{x} - l . \overline{n+1} . \frac{x \cos \delta + z \sin \delta}{r^2} \right\}.$$

In the direction of y

$$= \mathrm{I}\, m \frac{y}{r^{n+1}} \left\{ 1 - l . \overline{n+1} . \frac{x \cos \delta + z \sin \delta}{r^2} \right\}.$$

In the direction of z

$$= \mathrm{I}\, m . \frac{z}{r^{n+1}} \left\{ 1 + l . \frac{\sin \delta}{z} - l . \overline{n+1} . \frac{x \cos \delta + z \sin \delta}{r^2} \right\}.$$

The difference between the attractive and repulsive forces, or the true disturbing force, will be—In the direction of x,

$$- \mathrm{I} \cos \delta . \frac{2 l m}{r^{n+1}} + \mathrm{I} \cos \delta . \frac{2 . \overline{n+1} . l m . x^2}{r^{n+3}} + \mathrm{I} \sin \delta . \frac{2 . \overline{n+1} . l m . x z}{r^{n+3}}.$$

In the direction of y,

$$\mathrm{I} \cos \delta . \frac{2 . \overline{n+1} . l m . x y}{r^{n+3}} + \mathrm{I} \sin \delta . \frac{2 . \overline{n+1} . l m . y z}{r^{n+3}}.$$

In the direction of z,

$$- \mathrm{I} \sin \delta . \frac{2 l m}{r^{n+1}} + \mathrm{I} \cos \delta . \frac{2 . \overline{n+1} . l m . x z}{r^{n+3}} + \mathrm{I} \sin \delta . \frac{2 . \overline{n+1} . l m . z^2}{r^{n+3}}.$$

These are expressions for the disturbing forces produced by a single particle. To find the disturbing forces produced by the whole of the iron in the ship, we must take the sum of these expressions for every particle in the ship. Expressing this summation by the letter S, we have the whole disturbing forces as follows:

In the direction of x,

$$- \mathrm{I} \cos \delta . \mathrm{S} \frac{2 l m}{r^{n+1}} + \mathrm{I} \cos \delta . \mathrm{S} \frac{2 . \overline{n+1} . l m . x^2}{r^{n+3}} + \mathrm{I} \sin \delta . \mathrm{S} \frac{2 . \overline{n+1} . l m . x z}{r^{n+3}}.$$

In the direction of y,

$$\mathrm{I} \cos \delta . \mathrm{S} \frac{2 . \overline{n+1} . l m . x y}{r^{n+3}} + \mathrm{I} \sin \delta . \mathrm{S} \frac{2 . \overline{n+1} . l m . y z}{r^{n+3}}.$$

In the direction of z,

$$- \mathrm{I} \sin \delta . \mathrm{S} \frac{2 l m}{r^{n+1}} + \mathrm{I} \cos \delta . \mathrm{S} \frac{2 . \overline{n+1} . l m . x z}{r^{n+3}} + \mathrm{I} \sin \delta . \mathrm{S} \frac{2 . \overline{n+1} . l m . z^2}{r^{n+3}}.$$

We will now transform these formulæ by substituting for x, y, and z, their values in terms of r, A, a, and b. And we will suppose the compass to be in the vertical plane passing through the ship's keel, and the arrangement of the iron on both sides of that plane to be symmetrical. Then

$$\frac{x^2}{r^{n+3}} = \frac{\cos^2 b . \cos^2 \overline{\mathrm{A} + a}}{r^{n+1}} = \frac{1}{2} \frac{\cos^2 b}{r^{n+1}} + \frac{1}{2} \frac{\cos^2 b}{r^{n+1}} (\cos 2\,\mathrm{A} . \cos 2\, a - \sin 2\,\mathrm{A} . \sin 2\, a).$$

But

$$S\frac{lm\cos^2 b\,.\sin 2\,A\,.\sin 2\,a}{r^{n+1}},\ \text{or}\ \sin 2\,A\,.\,S\frac{lm\cos^2 b\,.\sin 2\,a}{r^{n+1}},$$

will evidently $= 0$, because for the same value of l, m, b, and r, there will be two values of a, one positive and the other negative. Hence the term

$$S\frac{2\,.\overline{n+1}\,.\,lm\,.\,x^2}{r^{n+3}}$$

will become

$$\overline{n+1}\,.\,S\frac{lm\,.\cos^2 b}{r^{n+1}} + \cos 2\,A\,.\,\overline{n+1}\,.\,S\frac{lm\,.\cos^2 b\,.\cos 2\,a}{r^{n+1}}.$$

The value of $\frac{xz}{r^{n+3}}$ is

$$\frac{\sin b\,.\cos b\,.\,(\cos A\,.\cos a - \sin A\,.\sin a)}{r^{n+1}}$$

in which, for the same reason, the term $\sin A\,.\sin a$ is to be rejected. Hence the term

$$S\frac{2\,.\overline{n+1}\,.\,lm\,.\,xz}{r^{n+3}}$$

will become

$$\cos A\,.\,2\,.\,\overline{n+1}\,.\,S\frac{lm\,.\sin b\,.\cos b\,.\cos a}{r^{n+1}}.$$

The value of $\frac{xy}{r^{n+3}}$ is

$$\frac{1}{2}\,\frac{\cos^2 b\,(\sin 2\,A\,.\cos 2\,a + \cos 2\,A\,.\sin 2\,a)}{r^{n+1}}.$$

Hence the term

$$S\frac{2\,.\overline{n+1}\,.\,lm\,.\,xy}{r^{n+3}}$$

will become

$$\sin 2\,A\,.\,\overline{n+1}\,.\,S\frac{lm\,.\cos^2 b\,.\cos 2\,a}{r^{n+1}}.$$

The value of $\frac{yz}{r^{n+3}}$ is

$$\frac{\sin b\,.\cos b\,(\sin A\,.\cos a + \cos A\,.\sin a)}{r^{n+1}}.$$

Hence the term

$$S\frac{2\,.\overline{n+1}\,.\,lm\,.\,yz}{r^{n+3}}$$

will become

$$\sin A\,.\,2\,.\,\overline{n+1}\,.\,S\frac{lm.\sin b.\cos b.\cos a}{r^{n+1}}.$$

The value of $\frac{z^2}{r^{n+3}}$ is $\frac{\sin^2 b}{r^{n+1}}$, which admits of no further reduction.

Hence the expressions for the whole disturbing forces are,—In the direction of x,

$$-\mathrm{I}\cos\delta\left\{\mathrm{S}\,\frac{2\,lm}{r^{n+1}}-\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\cos^2 b}{r^{n+1}}\right\}+\mathrm{I}\cos\delta\,.\cos 2\,\mathrm{A}\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\cos^2 b\,.\cos 2\,a}{r^{n+1}}$$

$$+\,\mathrm{I}\sin\delta\,.\cos\mathrm{A}\,.\,2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\sin b\,.\cos b\,.\cos a}{r^{n+1}}.$$

In the direction of y,

$$\mathrm{I}\cos\delta\,.\sin 2\,\mathrm{A}\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\cos^2 b\,.\cos 2\,a}{r^{n+1}}+\mathrm{I}\sin\delta\,.\sin\mathrm{A}\,.\,2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm.\sin b\,.\cos b.\cos a}{r^{n+1}}$$

In the direction of z,

$$-\mathrm{I}\sin\delta.\left\{\mathrm{S}\,\frac{2\,lm}{r^{n+1}}-2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\sin^2 b}{r^{n+1}}\right\}+\mathrm{I}\cos\delta\,.\cos\mathrm{A}.2.\overline{n+1}\,.\,\mathrm{S}\,\frac{lm.\sin b.\cos b.\cos a}{r^{n+1}}$$

Let

$$\mathrm{S}\,\frac{2\,lm}{r^{n+1}}-\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\cos^2 b}{r^{n+1}}=\mathrm{M}$$

$$2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\sin b\,.\cos b\,.\cos a}{r^{n+1}}=\mathrm{N}$$

$$\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\cos^2 b\,.\cos 2\,a}{r^{n+1}}=\mathrm{P}$$

$$\mathrm{S}\,\frac{2\,lm}{r^{n+1}}-2\,.\,\overline{n+1}\,.\,\mathrm{S}\,\frac{lm\,.\sin^2 b}{r^{n+1}}=\mathrm{Q}.$$

The four quantities M, N, P, Q, are then constant, depending solely upon the construction of the ship, not changing with any variations of terrestrial locality, or of magnetic dip or intensity. Then the disturbing forces (always estimated by their action on the north or marked end of the needle) are

Towards the magnetic north $-\mathrm{I}\cos\delta\,.\,\mathrm{M}+\mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\cos 2\,\mathrm{A}+\mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\cos\mathrm{A}$.

Towards the magnetic east . $\mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\sin 2\,\mathrm{A}+\mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\sin\mathrm{A}$.

Vertically downwards . . . $-\mathrm{I}\sin\delta\,.\,\mathrm{Q}+\mathrm{I}\cos\delta\,.\,\mathrm{N}\,.\cos\mathrm{A}$.

Before transforming these into another shape, we will consider the construction which they indicate as proper for the correction of a compass disturbed by the induced magnetism only of the iron in a ship.

The only force which it is necessary to destroy is that directed to the east, or

$$\mathrm{I}\cos\delta\,.\,\mathrm{P}\,.\sin 2\,\mathrm{A}+\mathrm{I}\sin\delta\,.\,\mathrm{N}\,.\sin\mathrm{A}.$$

In the usual cases (at least for wood-built ships) P and N will both have positive values; the iron being supposed to lie almost entirely on one side of the compass (so that $\cos a$ and $\cos 2\,a$ are almost always positive), and being almost entirely at a lower level than the compass (so that $\sin b\cos b$ is almost always positive).

Suppose now we find a mass of iron, which when placed at a certain distance and a certain depression, and in the same azimuth as the ship's head, will produce the same disturbance of the compass. (This is the method of determining the distance, &c. for Barlow's plate.) Then this mass, in the azimuth A, produces the disturbing

force to the east, $I \cos \delta . P . \sin 2 A + I \sin \delta . N . \sin A$; and therefore in the azimuth A' it produces the disturbing force to the east, $I \cos \delta . P . \sin 2 A' + I \sin \delta . N . \sin A'$. Suppose now that the mass is placed towards the stern of the ship (or on the side opposite to that in which the ship's iron is, for the most part, situated), but at the same distance and depression as those already determined. (This is the rule for applying BARLOW's plate as a partial corrector.) A' must now be made $= A + 180°$; and the disturbing force produced by the mass is now $I \cos \delta . P . \sin 2 A - I \sin \delta . N . \sin A$. Combining this with the disturbing force produced by the ship's iron, the compound force is reduced to $2 I \cos \delta . P . \sin 2 A$; that is, one of the terms expressing the disturbing force is destroyed, and the other is doubled. Whether this change of the force is advantageous or prejudicial, will depend not only on the value of δ (the dip), but also on the proportion between the values of N and P (that is, on the way in which the iron is distributed with regard to elevation above or depression below the compass, and with regard to the manner in which it surrounds the compass).

Let us consider, for instance, the case of a long wood-built steam-boat, with a compass on deck or in a cabin. It is probable that the iron of the engines and funnel may be pretty equally distributed above and below the level of the compass. Here $\sin b . \cos b$ has as many negative as positive values, and therefore $N = 0$. But $\cos 2 a$ is always positive, because the azimuth of any part of the iron from the vertical plane passing through the keel does not amount to 45°, and therefore P is positive. Consequently in this instance the application of BARLOW's plate doubles the error of the compass.

Yet it is possible in any case to destroy the disturbing force entirely. Suppose that BARLOW's plate is fixed as above mentioned, and that the disturbing force is therefore $2 I \cos \delta . P . \sin 2 A$. Now let a second mass of iron be introduced, with its centre at the same level as the compass, and in the azimuth A''. The general expression for the force which it produces is

$$I \cos \delta . P'' . \sin 2 A'' + I \sin \delta . N'' . \sin A''.$$

But $N'' = 0$ (because there are as many negative values of $\sin b . \cos b$ as there are positive). Combining this, then, with the force produced by the ship's iron and by the BARLOW's plate already mounted, the whole force is

$$I \cos \delta . (2 P . \sin 2 A + P'' . \sin 2 A'').$$

Now this quantity may be made $= 0$ by making $A'' = A \pm 90°$, and $P'' = 2 P$; for then

$$\sin 2 A'' = \sin (2 A \pm 180°) = - \sin 2 A,$$

and the second factor becomes

$$2 P . \sin 2 A - 2 P . \sin 2 A.$$

Hence we get the following simple rule for the perfect correction of the compass.

1. Determine the position of BARLOW's plate, with regard to the compass, which

will produce the same effect as the iron in the ship. (It will be sufficient if when placed on the E. side it produces the same effect as the ship when the ship's head is E.)

2. Fix BARLOW's plate at the distance and depression determined by the last experiment, but in the opposite azimuth (or towards the ship's stern).

3. Mount another mass of iron at the same level as the compass, but on the starboard or larboard side, and determine its position so that the compass shall point correctly when the ship's head is N.E., S.E., S.W., or N.W.

Then the compass will be correct in all positions of the ship's head, and in all magnetic latitudes.

When the disturbing iron of the ship is at the same level as the compass, the correction is much more simple. It is only necessary then to introduce a single mass of iron at the starboard or larboard side, and at the same level as the compass. For the ship's disturbing force is then

$$I \cos \delta \,.\, P \,.\, \sin 2\,A,$$

and the force produced by this mass is

$$I \cos \delta \,.\, P'' \,.\, \sin 2\,A'';$$

and if $A'' = A \pm 90°$, the sum of these terms will be 0, provided $P'' = P$, the distance for which condition will be easily ascertained by experiment.

In general it may be remarked, that if one mass of iron is placed exactly opposite another equal mass, both in azimuth and in elevation, it doubles its disturbing effect; if one mass be placed opposite the other in azimuth and at the same elevation or depression, or if it be placed in the same azimuth but with elevation instead of depression, or *vice versâ*, it destroys that term of the disturbance which depends on sin A, and doubles that which depends on sin 2 A. And if one mass be placed at the same level as the compass, its effects may be destroyed by placing another mass at the same level, in azimuth differing 90° on either side. (This conclusion, for spherical masses, was also obtained by POISSON.) To this we may add, that if a disturbance, from whatever cause arising, follows the law of + sin 2 A (changing sign in the successive quadrants, and positive when the ship's head is between N. and E.), it may be destroyed by placing a mass of iron on the starboard or larboard side, at the same level as the compass; if it follows the law of − sin 2 A, the mass of iron must be on the fore or aft side.

The form of BARLOW's plate appears objectionable, in so far as having its broad side turned towards the compass, it occupies a considerable arc of azimuth, and cos 2 a may therefore (for some parts of it) be small or negative. A plate of iron rolled into a scroll, and placed with its end directed towards the compass, appears better; but I prefer a long box filled with iron chain, as less likely to possess the permanent magnetism, from which no plate iron is free; its end should be directed towards the compass.

I trust that in the preceding remarks on the imperfection of BARLOW's plate as a

corrector, I shall not appear to have treated Mr. BARLOW's construction with harshness. I can truly assert that my feelings and my intentions are of a very different character. To Mr. BARLOW we are indebted for almost all the experimental knowledge which we possess on the subject of the disturbance produced by masses of iron; the use of his plate *as a corrector* was avowedly proposed by him as imperfect; and it requires no great experience in the pursuit of experimental and practical philosophy to learn to venerate the man who makes the first step in devising a construction applicable to a given purpose. I must not omit to add, that Mr. BARLOW's plate corrects that part of the disturbance which is most important in the most critical circumstances, namely, in the high magnetic latitudes. Without Mr. BARLOW's proposed construction I should never have arrived at the more perfect construction described above; with it the invention of something more perfect was easy.

We will now resume the consideration of the expressions for the disturbing forces produced by the ship.

The first term in the expression for the disturbance towards the north is constant. It appears therefore that if M be positive, the absolute directive force on the needle will be, on the whole, diminished. On examining the expression for M it will be seen that its value is greatest (for a given mass of iron at a given distance) when the iron is immediately above or below the compass, and that it is least when the iron is at the same level as the compass. It is proper therefore that both in the construction of the ship and in the fixing of correctors, no large mass of iron should be placed below the compass.

The expression for disturbing force towards the ship's head is,

$$\cos A \times \text{force towards north} + \sin A \times \text{force towards east},$$

$$= I \cos \delta . (- M + P) \cos A + I \sin \delta . N.$$

The second term of this expression is independent of the position of the ship, and therefore cannot be distinguished, in experiments made at any one locality, from permanent magnetism. But as it contains the factor $I \sin \delta$, or the vertical force of terrestrial magnetism, it may be discovered from experiments made in different localities, where the magnitude of the vertical force differs much.

The expression for the disturbing force towards the starboard side of the ship is

$$\cos A \times \text{force towards east} - \sin A \times \text{force towards north},$$

$$= I \cos \delta . (M + P) \sin A.$$

It will be convenient to remember that M is the coefficient on which the absolute diminution of the directive force depends; that N is the coefficient upon which depends the force similar to permanent magnetism; and that P is the coefficient upon which depends the transversal force changing sign in the successive quadrants of azimuth.

It will also be convenient to remember that M is largest when the mass of iron is below, and may be negative if the mass of iron is nearly at the same level; that N

will vanish if the mass of iron is nearly at the same level as the compass, or may vanish from the opposition of different masses in azimuth while their depression is similar; and that P will vanish if the mass of iron is below, or may vanish from the opposition of effect produced by masses in azimuths differing 90°, but that masses in opposite azimuths combine to increase P.

It may now be desirable to consider the modifications produced in the horizontal forces by the heeling of the ship. The pitching causes a variation in the position of the keel alternately in one direction and in the opposite, and its effects may therefore probably be neglected; but the heeling frequently continues in the same direction for many hours or days, and therefore it will not be safe to omit it. To simplify our expressions, we will suppose the angle of heel to be not very great.

Let the different particles of the ship be referred to the place of the compass by means of the angles a and b as before, but let a be the azimuth of any particle from the direction of the ship's keel as measured in a plane parallel to that of the deck, and let b be the angle of depression as measured, from the plane parallel to the deck, towards the line which is normal to the deck. Then a and b are independent of the ship's heeling. Let h be the angle of heel towards the starboard side; and suppose h so small that its square may be neglected. Then we have

$$x = r \,.\, \cos b \,.\, \cos a \,.\, \cos \mathrm{A} - r \,.\, \cos b \,.\, \sin a \,.\, \sin \mathrm{A} + \sin h \,.\, r \,.\, \sin b \,.\, \sin \mathrm{A}.$$

$$y = r \,.\, \cos b \,.\, \cos a \,.\, \sin \mathrm{A} + r \,.\, \cos b \,.\, \sin a \,.\, \cos \mathrm{A} - \sin h \,.\, r \,.\, \sin b \,.\, \cos \mathrm{A}.$$

$$z = r \,.\, \sin b + \sin h \,.\, r \,.\, \cos b \,.\, \sin a.$$

The new term introduced into x^2 is

$$\sin h \,.\, 2 r^2 \{\sin b \,.\, \cos b \,.\, \cos a \,.\, \sin \mathrm{A} \,.\, \cos \mathrm{A} - \sin b \,.\, \cos b \,.\, \sin a \,.\, \sin^2 \mathrm{A}\},$$

and therefore the new term introduced into

$$\mathrm{I} \cos \delta \,.\, \mathrm{S} \frac{2 \,.\, \overline{n+1} \,.\, l m \,.\, x^2}{r^{n+3}}$$

will be

$$\sin h \,.\, \mathrm{I} \cos \delta \,.\, \sin 2 \mathrm{A} \,.\, \mathrm{S} \frac{2 \,.\, \overline{n+1} \,.\, l m \,.\, \sin b \,.\, \cos b \,.\, \cos a}{r^{n+1}} = \sin h \,.\, \mathrm{I} \cos \delta \,.\, \mathrm{N} \,.\, \sin 2 \mathrm{A}.$$

The new term introduced into $x z$ will be

$$\sin h \,.\, r^2 \{\cos^2 b \,.\, \sin a \,.\, \cos a \,.\, \cos \mathrm{A} - \cos^2 b \,.\, \sin^2 a \,.\, \sin \mathrm{A} + \sin^2 b \,.\, \sin \mathrm{A}\},$$

and therefore the new term introduced into

$$\mathrm{I} \sin \delta \,.\, \mathrm{S} \frac{2 \,.\, \overline{n+1} \,.\, l m \,.\, x z}{r^{n+3}}$$

will be

$$\sin h \,.\, \mathrm{I} \sin \delta \,.\, \sin \mathrm{A} \,.\, \mathrm{S} \frac{2 \,.\, \overline{n+1} \,.\, l m \,.\, (\sin^2 b - \cos^2 b \,.\, \sin^2 a)}{r^{n+1}}.$$

Let

$$\mathrm{S} \frac{2 \,.\, \overline{n+1} \,.\, l m \,(\sin^2 b - \cos^2 b \,.\, \sin^2 a)}{r^{n+1}} = \mathrm{R};$$

(it is easily seen that $R = M + P - Q$); then the new term introduced into

$$I \sin \delta . S \frac{2 . \overline{n+1} . lm . xz}{r^{n+3}} \text{ is}$$

$$\sin h . I \sin \delta . R . \sin A.$$

Hence the whole addition to the force towards the magnetic north is

$$\sin h . I \cos \delta . N . \sin 2A + \sin h . I \sin \delta . R . \sin A.$$

The new term introduced into xy is

$\sin h . r^2 \{ - \sin b . \cos b . \cos a (\cos^2 A - \sin^2 A) + 2 \sin b . \cos b . \sin a . \sin A . \cos A \}$,

and therefore the new term introduced into

$$I \cos \delta . S \frac{2 . \overline{n+1} . lm . xy}{r^{n+3}}$$

will be

$$- \sin h . I \cos \delta . \cos 2A . S \frac{2 . \overline{n+1} . lm . \sin b . \cos b . \cos a}{r^{n+1}} = - \sin h . I \cos \delta . N . \cos 2A.$$

The new term introduced into yz is

$$\sin h . r^2 \{ \cos^2 b . \sin a . \cos a . \sin A + \cos^2 b . \sin^2 a . \cos A - \sin^2 b . \cos A \},$$

and therefore the new term introduced into

$$I \sin \delta . S \frac{2 . \overline{n+1} . lm . yz}{r^{n+3}}$$

will be

$$\sin h . I \sin \delta . \cos A . S \frac{2 . \overline{n+1} . lm . (\cos^2 b . \sin^2 a - \sin^2 b)}{r^{n+1}} = - \sin h . I \sin \delta . R \cos A.$$

Hence the whole addition to the force towards the magnetic east is

$$- \sin h . I \cos \delta . N . \cos 2A - \sin h . I \sin \delta . R . \cos A.$$

Combining the expressions just found with those found in the first investigation of forces, we have the following:

Whole disturbing force to magnetic north $= - I \cos \delta . M + I \cos \delta . P . \cos 2A$
$+ I \sin \delta . N . \cos A + \sin h \{ I \cos \delta . N . \sin 2A + I \sin \delta . R . \sin A \}$.

Whole disturbing force to magnetic east $= I \cos \delta . P . \sin 2A + I \sin \delta . N . \sin A$
$+ \sin h . \{ - I \cos \delta . N . \cos 2A - I \sin \delta . R . \cos A \}$.

Transforming these into expressions for the forces in directions related to the direction of the ship's keel, we have

Whole disturbing force towards ship's head

$= - I \cos \delta . M . \cos A + I \cos \delta . P . \cos A + I \sin \delta . N + \sin h . I \cos \delta . N . \sin . A.$

Whole disturbing force in the horizontal plane, towards the starboard side

$= I \cos \delta . M . \sin A + I \cos \delta . P . \sin A + \sin h \{ - I \cos \delta . N . \cos A - I \sin \delta . R \}.$

We have already shown how the terms independent of h (except those multiplied

by M) may be neutralized. One part of the operation was, to place aft the compass a mass of iron which if in front of the compass would have given the same value of N which the ship gives. Now when this mass of iron is fixed aft, we may either regard it as an additional part of the ship in the same azimuth A and having the same angle of heel h, but giving a constant N negative in magnitude by reason of the factor $\cos a = \cos 180°$ which enters into its formation; or we may regard it as a new mass, with positive coefficient N, with azimuth $A + 180°$, with angle of heel $-h$, and whose action is estimated in the resulting expressions as towards the azimuth $A + 180°$ or from the ship's head. In either way of considering it, we find that by the process for neutralizing the term depending on N when the ship is not heeling, we have also neutralized the terms depending on N when the ship is heeling: and these terms may at once be put out of consideration. The only term, therefore, which remains to be considered is the following:

Disturbing force in the horizontal plane towards the starboard side, depending on the ship's heeling, $= -\sin h \,.\, I \sin \delta \,.\, R$.

For the *general* correction of this term, no easy method suggests itself. But the following considerations may serve to show the probability that constructions already proposed for another purpose will in a great measure correct it. First, with regard to the order of magnitude of this term; as $R = M + P - Q$, it is a term of the same order as the others which occur in this investigation (before multiplication by the factor $\sin h$), and therefore such masses of iron as those which are competent to correct the other terms may be expected to be of sufficient magnitude, with proper arrangement, to correct this term. Next, with regard to its law as depending on the position of the particles of iron: R is positive if $\sin^2 b$ exceeds $\cos^2 b \,.\, \sin^2 a$: but $r \,.\, \sin b$ is the depression of any particle below the compass measured perpendicularly to the deck, and $r \,.\, \cos b \,.\, \sin a$ is the ordinate measured from the plane passing through the keel and masts towards the starboard side. Hence we find this rule. If a line parallel to the keel be drawn through the compass, and if two planes be drawn through this line, inclined at angles of 45° to the plane passing through the masts and keel, so that a transverse section of the ship at any place present the section X: then all the iron in the upper and lower angles tends to increase R, and all the iron in the starboard and larboard angles tends to diminish R.

The principal part of the ship's iron will probably be in the lower angle, and will, therefore, tend to make R positive. Now we have, for correction of the term P, proposed to place a mass of iron on the starboard or larboard side, at the same level as the compass. It is evident that this iron tends to make R negative, and, therefore, tends to correct the effect of the ship's iron in disturbing the compass while the ship is heeling. Whether by varying the position of the mass of iron used to neutralize N, and thereby varying the magnitude both of P and of R, it may be practicable to make this lateral mass of iron neutralize both P and R, I cannot say. If it be impracticable, the accurate neutralization of P is evidently to be secured in preference

to that of R. In any case, however, it will be possible to neutralize R, or the remaining part of R, for any given magnetic latitude, by placing a magnet in a position perpendicular to the ship's deck, and with its centre at the same level as the centre of the compass. To secure rigorously the latter condition, the place of the magnet should be fore or aft the compass. If we suppose the marked end of the magnet to be downwards, its action downwards parallel to the mast may be represented by $-V'$: and when the ship heels, its action to the starboard side will be $V' . \sin h$: and the whole disturbing force towards the starboard side will, therefore, be $\sin h\ (V' - I \sin \delta . R)$, which by proper determination of the magnet's distance (since V' may be increased or diminished in almost any proportion) may be made $= 0$. If R or the remaining part of R is negative, the marked end of the magnet must be upwards. In consequence of the multiplication of one of the terms by $\sin \delta$, the correction thus made will be good for only one magnetic latitude.

The proportion which the term $\sin h . I \sin \delta . R$ bears to the directive force or $I \cos \delta$, being expressed by the fraction $\sin h . \tan \delta . R$, it is evident that this term, in high magnetic latitudes, may become important. I would, therefore, recommend that, when opportunity serves, an attempt should be made to determine the position of a magnet which will neutralize R. This can be done, so far as I see, only by trial with the ship's masts considerably inclined. Such an operation would probably be too troublesome in icy seas: but it might be possible to make the necessary observations in England, and then (by trial of the powers of the magnet at different distances) to define the relation between the distance of the magnet and the dip of the dipping needle, so that, knowing the dip, the magnet could be placed at once in the position in which it will neutralize R. A graduated slider to carry the magnet would make the practical use of this method very simple.

Section IV.—*Combination of Induced Magnetism with Permanent Magnetism.*

It is supposed in the following investigations that the existence of permanent magnetism produces no modification in the induced magnetism, their effects being simply combined by algebraical addition.

Whatever be the number or direction of the magnets entering into the composition of the ship, their effects on the compass may be represented by three forces: namely, one directed to the ship's head, one directed to the starboard side, and one vertically downwards. We will designate these by the letters H, S, and V, respectively.

The resolved parts of the two horizontal forces in the north and east directions are respectively $H \cos A - S \sin A$, and $H \sin A + S \cos A$. If we combine these with the expressions found for the disturbance produced in the same directions by induced magnetism, we obtain expressions which are rather long and troublesome to use. It will be better to use the expressions for the disturbing forces directed to the head and to the starboard side. We have then

Whole disturbing force towards the ship's head

$$= H + I \cos \delta . (- M + P) . \cos A + I \sin \delta . N.$$

Whole disturbing force towards the starboard side

$$= S + I \cos \delta . (M + P) . \sin A.$$

We shall now point out the way in which the numerical values of these quantities may be found from experiment.

A needle, delicately suspended, being made to vibrate horizontally, and the time occupied by a certain number of vibrations being noted, in a place on shore free from local disturbance, and also in the place of the ship's compass in any position of the ship's head, the ratio of the whole intensity of the horizontal force in the place of the ship's compass to the intensity on shore ($I \cos \delta$) is known, being the inverse ratio of the squares of the times of vibration. This force, however, is not directed to the north, but is in the disturbed direction of the needle of the ship's compass. The amount of the needle's angular disturbance towards the east being found by observation, the intensity just found must be multiplied by the cosine of the angular disturbance to give the resolved part of the force directed to the north, and by the sine of the angular disturbance to give the resolved part of the force directed to the east. The former of these, diminished by the intensity on shore, gives the actual disturbing force to the north: the latter, unaltered, gives the actual disturbing force to the east. The disturbing forces to the head and to the starboard side may now be calculated numerically, by giving numerical values to every part of these formulæ:

Disturbing force to head

$$= \cos A \times \text{disturbing force to north} + \sin A \times \text{disturbing force to east.}$$

Disturbing force to starboard side

$$= \cos A \times \text{disturbing force to east} - \sin A \times \text{disturbing force to north.}$$

These calculations are to be made for every position of the ship in which the vibrations have been observed. When $A = 0°, 90°, 180°, 270°$, the factors are 0 or 1: when $A = 45°, 135°, 225°, 315°$, the factors are equal in magnitude: in other positions of the ship, the numerical expressions for the factors are not so simple.

The calculations being performed, we have now two series of numbers corresponding to a series of different values of A; the numbers of one series are to represent (errors of observation excepted) different values of

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) + (- M + P) \cos A,$$

and the numbers of the other series are to represent different values of

$$\frac{S}{I \cos \delta} + (M + P) \sin A$$

and from these conditions the several constants

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right), \qquad (- M + P), \qquad \frac{S}{I \cos \delta}, \qquad \text{and } (M + P)$$

are to be determined. This may be done by any of the methods used in astronomical or physical inquiries for determining the numerical values of the constants in a given formula which will best satisfy a number of equations of condition. The values of the constants, so found, being substituted in each equation, the agreement of the result with the number deduced from observation, within the limits of errors of observation, may be considered as a general proof of the correctness of the theory and of the accuracy of the numerical operation.

One of the constants thus found is $\frac{H}{I \cos \delta} + \tan \delta \,.\, N$. We have no means of determining separately the parts of which it consists, by observations made at one place. In a wood-built ship, where the various irons are laid in all possible positions, and where a great proportion consists of cast-iron (which does not appear usually to possess permanent magnetism) it is probable that the term $\frac{H}{I \cos \delta}$ is insignificant. In an iron-built ship we can conjecture the value of N from the values found for M and P: it will appear probable, from the subsequent investigations, that N is small, while $\frac{H}{I \cos \delta}$ may be very large.

From the values of $(-M + P)$ and $(M + P)$, the values of M and P will be immediately obtained.

We will now proceed with the consideration of the means of correcting the compass, so that its needle shall always point truly north. A due consideration of the preceding theory will show that this may be effected by a magnet or combination of magnets, and by a mass of iron, at the same level as the compass, placed on the starboard or larboard side if P is positive, or on the fore or aft side if P is negative.

Let H' be the force of the correcting magnets directed towards the head; S' the force directed to the starboard side; A' the azimuth of the mass of iron; P' the coefficient for this mass corresponding to P for the ship; M' and N' will be $= 0$. The forces produced by the mass of iron will be

Towards the north . . . . $I \cos \delta \,.\, P' \,.\, \cos 2\, A'$.
Towards the east . . . . $I \cos \delta \,.\, P' \,.\, \sin 2\, A'$.

From which the following are obtained:

Force towards the head . . . $= I \cos \delta \,.\, P' \,.\, \cos (2\, A' - A)$.
Force towards the starboard side $= I \cos \delta \,.\, P' \,.\, \sin (2\, A' - A)$.

And if the mass be on the starboard or larboard side of the compass, $A' = A \pm 90°$, and the expressions become

Force towards the head . . . $= - I \,.\, \cos \delta \,.\, P' \,.\, \cos A$.
Force towards the starboard side $= - I \,.\, \cos \delta \,.\, P' \,.\, \sin A$.

Combining these forces and those produced by the correcting magnets with the forces produced by the induced and permanent magnetism of the ship, we have the following:

Disturbing force towards the ship's head

$$= (H + I \sin \delta . N + H') + I \cos \delta (- M + P - P') \cos A.$$

Disturbing force towards the starboard side

$$= (S + S') + I \cos \delta (M + P - P') \sin A.$$

The constant terms will be destroyed by making $H' = - (H + I \sin \delta . N)$, or $\frac{H'}{I \cos \delta} = - \left(\frac{H}{I \cos \delta} + \tan \delta . N\right)$ and $\frac{S'}{I \cos \delta} = - \frac{S}{I \cos \delta}$; which determine (in terms of quantities found from the experiments) the ratios which the directive forces of the magnet or magnets introduced are to bear to the terrestrial directive force. If N have a sensible value, the value of H′ here found is strictly correct for one magnetic latitude only. The distance at which a given magnet must be placed will be determined on shore by placing it transverse to the meridian, and determining by trial the distance from a compass at which it makes the tangent of the deviation equal to the ratio required.

In the variable terms, the utmost that we can do is to make $P - P' = 0$, or $P' = P$. The easiest way of determining the distance of the mass of iron which shall produce this effect is by experiment on the angular deviation which it will produce. For the effect depending on P only produces a force towards the east $= I \cos \delta . P . \sin 2 A$ · the terrestrial directive force is $I \cos \delta$; therefore the deviation (supposed small,) of a compass otherwise undisturbed, is, in terms of radius, $P . \sin 2 A$; or, in degrees, $57^{\circ}\cdot 3 \times P . \sin 2 A$. If $A = 45^{\circ}$, this is $57^{\circ}\cdot 3 \times P$. But P is known from the experiments. Hence we have this rule. Having a compass on shore, place the mass of iron, which is to be mounted for corrector, at the level of the compass, and in azimuth 45°; find by trial the distance at which it will cause the compass to deviate through $57^{\circ}\cdot 3 \times P$; that is the distance from the ship's compass at which it must be mounted on the starboard or larboard side.

There still remain uncorrected the following forces:

Towards the ship's head $- I \cos \delta . M . \cos A.$

Towards the starboard side $I \cos \delta . M . \sin A.$

From these we obtain,

Remaining disturbing force towards the north $- I \cos \delta . M.$

Remaining disturbing force towards the east $0.$

The direction of the needle, therefore, is not disturbed; these terms express only the diminution of the terrestrial directive force of which we have already spoken.

If it be desired to correct the compass accurately for all magnetic latitudes, the following is the course which must be pursued.

1. From experiments similar to those already described, determine in two different magnetic latitudes the numerical values of

$$\frac{H}{I \cos \delta} + \tan \delta . N, \text{ and } \frac{H}{I' \cos \delta'} + \tan \delta' . N:$$

and from these find H and N.

2. From experiments on a compass on shore, with a mass of iron not in the same level, and in azimuth 90°, determine a position in which it will produce the deviation $57^{\circ}\cdot3 \times \tan\delta \,.\, N$. This is a position in which it is to be fixed in the ship; on the aft side of the compass if N is positive, or the fore side if N is negative.

3. With the mass of iron at that position, try the deviations which it produces on a compass on shore in azimuth 45° and 135°, and take half the algebraical excess of the former above the latter. This is to be added to the angle $57^{\circ}\cdot3 \times P$, to produce the angle which the other mass on the starboard or larboard side is to correct.

4. The magnet introduced as corrector must have an intensity H′ equal to − H.

It may be proper now to say a few words relative to the application of our theory to the observations of the dipping-needle, as made on the deck of the Rainbow.

The resolved part of the terrestrial force parallel to the ship's keel is $I \cos\delta \,.\, \cos A$, and the resolved part vertically downwards is $I \sin\delta$. Combining these with the forces in those directions produced by the permanent and induced magnetism in the ship, we have

$$\text{Cotangent of dip towards ship's head} = \frac{\text{Force towards ship's head}}{\text{Force vertically downwards}}.$$

$$= \frac{I\cos\delta \,.\, \cos A + H + I\sin\delta \,.\, N + I\cos\delta \,.\, (-M + P)\cos A}{I\sin\delta + V - I\sin\delta \,.\, Q + I\cos\delta \,.\, N \,.\, \cos A}$$

$$= \frac{\cot\delta\,(1 - M + P)\cos A + \cot\delta\left(\frac{H}{I\cos\delta} + \tan\delta \,.\, N\right)}{1 + \frac{V}{I\sin\delta} - Q + \cot\delta \,.\, N \,.\, \cos A}.$$

Let c be the observed cotangent of the dip towards the ship's head; the equation then gives

$$\cot\delta\,(1 - M + P)\cos A + \cot\delta\left(\frac{H}{I\cos\delta} + \tan\delta \,.\, N\right) = c\left(1 + \frac{V}{I\sin\delta} - Q\right)$$
$$+ c \,.\, \cot\delta \,.\, N\cos A.$$

Substituting in this the values of $(-M + P)$ and $\left(\frac{H}{I\cos\delta} + \tan\delta \,.\, N\right)$ found from the observations of intensity, we shall be able to ascertain the values of $1 + \frac{V}{I\sin\delta} - Q$ and N. Where the dipping-needle is incorrect, the best value of δ will be the incorrect dip as shown by the needle.

In like manner we find

$$\text{Cotangent of dip towards starboard side} = \frac{-I\cos\delta \,.\, \sin A + S + I\cos\delta \,.\, (M + P)\sin A}{I\sin\delta + V - I\sin\delta \,.\, Q + I\cos\delta \,.\, N \,.\, \cos A}$$

$$= \frac{-\cot\delta \,.\, (1 - M - P) \,.\, \sin A + \cot\delta \,.\, \frac{S}{I\cos\delta}}{1 + \frac{V}{I\sin\delta} - Q + \cot\delta \,.\, N \,.\, \cos A}.$$

Let c' be the observed cotangent of the dip towards the starboard side; the equation then gives

$$-\cot\delta\,(1 - M - P)\,.\sin A + \cot\delta\,.\,\frac{S}{I\cos\delta} = c'\left(1 + \frac{V}{I\sin\delta} - Q\right) + c'.\cot\delta\,.\,N\,.\cos A,$$

which will assist, in the same manner as the last, to give values of $1 + \frac{V}{I\sin\delta} - Q$ and N.

With regard to the effect of the ship's heeling, the following investigation appears sufficient. Supposing a magnet to be introduced, parallel to the masts, and with its marked end downwards, its effect downwards and parallel to the masts will be represented by $-V'$. Combining this with the force V of the ship in the same direction, and supposing the angle of heel to starboard to be h, we have for the force to starboard in a horizontal plane

$$\sin h\,(V' - V).$$

Combining these with the forces found in Section III, we have

Force towards ship's head, depending on the heeling,

$$\sin h\,.\,I\cos\delta\,.\,N\,.\sin A.$$

Force towards starboard side, depending on the heeling,

$$\sin h\,(V' - V - I\cos\delta\,.\,N\,.\cos A - I\sin\delta\,.\,R).$$

As we cannot ensure the neutralization of the term N by the induced magnetism of another mass of iron, (except by experiments in different magnetic latitudes) we must suppose it to exist uncorrected. It is probable that the effect of the forces depending on it will not be great. Their proportion to the directive force will be

$$\sin h\,.\,N\,.\sin A \text{ for the force to the head,}$$

and $-\sin h\,.\,N\,.\cos A$ for the force to the starboard side; or

$$-\sin h\,.\,N\,.\cos 2\,A$$

for the force to the magnetic east. This proportion does not increase in the high magnetic latitudes.

The proportion of the remaining force $\sin h\,(V' - V - I\sin\delta\,.\,R)$ to the directive force, which is expressed by $\sin h\left(\frac{V' - V}{I\cos\delta} - \tan\delta\,.\,R\right)$, becomes great in the high magnetic latitudes, and this force therefore ought not to be neglected. From the observations with the dipping-needle, as mentioned above, we may probably obtain the value of V, or rather $V - I\sin\delta\,.\,Q$, and may, therefore, determine V' so as partially to neutralize it. Still the term $\tan\delta\,.\,R$ remains: and therefore the correction would be imperfect. It would probably be best, therefore, to attempt the correction tentatively (as mentioned before, for wood-built ships). The vessel being placed with her head to the north, and made to heel through the angle h, and the place of a given

magnet (arranged as before mentioned) being determined so as to correct any new deviation depending on the heeling, V' will $= V + I \cos \delta . N + I \sin \delta . R$. As we cannot easily separate the different terms of this expression, there appears no way of providing for the correction in different magnetic latitudes but by trial in each.

If V should have a sensible magnitude, the principal part of the error of the compass at any place of the ship, when the ship is heeling, will depend on V. If it appears from other of the investigations, that V is small, we may be assured that the deviation depending upon the ship's heeling will not, except in high latitudes, be very great.

Section V.—*Application of the Theory to the Observations made in the Rainbow.*

As the disturbance of the compass was not observed in the experiments for intensity, it has been interpolated graphically between those previously observed. From the times of vibration given in Section II. the following numbers are computed by the methods of Section IV.

No. of station.	Azimuth of ship's head.	Disturbance of compass to East.	Intensity to N. / I cos δ	Disturbing force to N. / I cos δ	Disturbing force to E / I cos δ	Dist. force to ship's head / I cos δ	Dist. force to starb. side / I cos δ
	°	° ′					
I.	0	− 38 0	+ 0·18	− 0·82	− 0·13	− 0·82	− 0·13
	90	− 33 50	+ 1·15	+ 0·15	− 0·77	− 0·77	− 0·15
	180	+ 6 0	+ 1·83	+ 0·83	+ 0·19	− 0·83	− 0·19
	270	+ 45 30	+ 0·77	− 0·23	+ 0·79	− 0·79	− 0·23
II.	0	− 17 0	+ 0·68	− 0·32	− 0·21	− 0·32	− 0·21
	90	− 14 0	+ 1·14	+ 0·14	− 0·28	− 0·28	− 0·14
	180	+ 9 20	+ 1·34	+ 0·34	+ 0·22	− 0·34	− 0·22
	270	+ 24 0	+ 0·73	− 0·27	+ 0·32	− 0·32	− 0·27
III.	0	− 12 30	+ 0·82	− 0·18	− 0·18	− 0·18	− 0·18
	90	− 12 40	+ 1·14	+ 0·14	− 0·26	− 0·26	− 0·14
	180	+ 7 20	+ 1·32	+ 0·32	+ 0·17	− 0·32	− 0·17
	270	+ 23 0	+ 0·72	− 0·28	+ 0·31	− 0·31	− 0·28
IV.	0	− 9 45	+ 0·89	− 0·11	− 0·15	− 0·11	− 0·15
	90	− 7 10	+ 1·15	+ 0·15	− 0·14	− 0·14	− 0·15
	180	+ 8 30	+ 1·18	+ 0·18	+ 0·18	− 0·18	− 0·18
	270	+ 10 15	+ 0·86	− 0·35	+ 0·12	− 0·12	− 0·35

It is impossible to glance at the numbers in the two last columns without noticing the smallness of the variation in each of the disturbing forces, as referred to lines of the ship, while the ship was turned round. From the moment when this Table was completed, it was perfectly clear that the explanation of the principal part of the disturbances of the compass was to be sought in the permanent magnetism of the ship.

Confining ourselves for the present to Station I., we have by the formulæ of the last section,

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) + (- M + P) = - 0{\cdot}82$$

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) = - 0{\cdot}77$$

$$\left(\frac{H}{I \cos \delta} + \tan \delta . N\right) - (- M + P) = - 0{\cdot}83$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}79$$

$$\frac{S}{I\cos\delta} = -0{\cdot}13$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}15$$

$$\frac{S}{I\cos\delta} = -0{\cdot}19$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}23$$

The most probable values appear to be

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}80$$

$$-M + P = 0{\cdot}00$$

$$\frac{S}{I\cos\delta} = -0{\cdot}17$$

$$M + P = 0{\cdot}04$$

whence

$$M = 0{\cdot}02, \qquad P = 0{\cdot}02.$$

The two last quantities are so small that the experiments can hardly be supposed to give their precise values. The value of M indicates that, when the direction is corrected, the intensity will be enfeebled $\frac{1}{50}$ part: that of P shows that, supposing the direction corrected by magnets only, there will be a deviation of the compass changing sign at the alternate quadrants, whose maximum is little more than 1°, and which is such that the needle turns to the right, or observed azimuths appear too small, when the azimuth of the ship's head is between 0 and 90°.

To prove these results completely, the following calculations were made. The two permanent magnetic forces represented by $-0{\cdot}80$ parallel to the ship's keel, and $-0{\cdot}17$ transverse to it, may be compounded into one force $-0{\cdot}82$, making an angle α with the ship's keel, where $\tan\alpha = \frac{17}{80}$ (the horizontal part of terrestrial magnetism being represented by 1·00). A line being taken to represent 1·00, with one end of this line for centre and with radius 0·82, a circle was described; upon this circle the angle α was graphically determined, and the different angles $A + \alpha$ were laid down (A being the azimuth of the ship's head) for every observation from No. 1. to 34. Each of the points on the circle thus determined was joined with the other end of the line, whose length = 1·00. It is clear that, if the force acting on the north end of the compass be the force compounded of terrestrial magnetism invariable in direction and magnitude, and of ship's magnetism invariable in magnitude, but changing direction with the ship, the resulting actual force will be represented by the joining

line last drawn. The angles made by that joining line with the line 1·00 ought, therefore, to be the same as the observed disturbance of compass, or if there is any certain difference, that difference ought to be explainable by the term P mentioned above. The following table contains the values of the angles made by the joining line with the line 1·00 (as taken from the graphical construction), or theoretical disturbance of the compass, compared with the observed disturbance; the sign + denotes that the needle is turned to the left.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
	° ′	° ′	° ′	° ′		° ′	° ′	° ′	° ′
1	203 5	−16 50	−15 50	−1 0	19	1 35	+40 45	+43 0	−2 15
2	214 53	−22 45	−21 0	−1 45	20	19 25	+51 20	+55 0	−3 40
3	220 0	−23 20	−23 25	+0 5	21	38 25	+50 50	+52 40	−1 50
4	230 0	−30 5	−27 40	−2 25	22	57 45	+46 30	+47 5	−0 35
5	240 50	−34 5	−32 20	−1 45	23	76 55	+39 15	+39 50	−0 35
6	248 50	−36 40	−35 35	−1 5	24	93 10	+33 5	+33 5	0 0
8	256 4	−40 2	−38 30	−1 32	25	109 0	+26 40	+26 15	+0 25
9	267 44	−44 27	−43 15	−1 12	26	125 50	+19 40	+19 0	+0 40
10	276 12	−47 50	−46 30	−1 20	27	142 10	+11 55	+11 50	+0 5
11	282 57	−49 27	−48 35	−0 52	28	159 10	+ 4 20	+ 4 0	+0 20
12	292 17	−52 25	−51 25	−1 0	29	171 5	− 1 35	− 1 20	−0 15
13	294 0	−53 45	−51 50	−1 55	30	189 45	−11 0	− 9 55	−1 5
14	300 35	−54 50	−53 40	−1 10	31	202 5	−16 45	−15 25	−1 20
15	308 0	−53 50	−54 40	+0 50	32	220 25	−25 10	−23 40	−1 30
16	313 47	−54 2	−54 50	+0 48	33	248 55	−37 50	−35 40	−2 10
17	321 40	−50 10	−53 40	+3 30	34	278 15	−48 25	−47 10	−1 15
18	338 25	−33 0	−35 30	+2 30					

It is quite evident from this table, first, that almost the whole disturbance of the compass is accounted for by the permanent magnetism; secondly, that the residual part follows with sufficient approximation the law of changing signs at the successive quadrants (modified in a small degree by the disturbance in the needle's direction), and with a maximum value not much different from the value 1° already predicted from the observations of intensity. In the quadrants adjacent to the azimuth 0° the maximum appears greater than this quantity, as it ought to appear, in consequence of the great diminution in the needle's directive power.

There remained only, for the complete verification of the theory, to effect an actual correction of the compass. With a two-feet bar magnet placed transversely to the magnetic meridian, experiments were made at Greenwich for ascertaining the distance at which it must be placed below a compass in order to make the needle deviate through 39° 25′ (the angle whose natural tangent = 0·82, the proportion of the ship's permanent magnetic force to the terrestrial directive force at Deptford). The magnet*

* I know not whether the following observation possesses any novelty or interest. A pair of 2-feet bar magnets had been constructed for me a year or more before this time by Messrs. Watkins and Hill. On trial, one was found to have not more than half the intensity of the other: it was retouched by the makers: it was again found to be feeble, was again touched; and had been laid up with its fellow and with the keepers on for some months. When trials were made for ascertaining the distance at which the magnets intended for

was then placed in a groove, cut in a board, and making with the edge of the board the angle whose tangent $= \frac{17}{80}$. This mode of mounting was adopted for facility of fixing in the ship, as it was then necessary only to make the edge of the board parallel to the ship's keel, the magnet being at the proper distance below the compass. A roll of iron plate was also prepared, tolerably free from permanent magnetism, and experiments were made to ascertain the distance from a compass at which in azimuth 45° it would cause a deviation something greater than 1°; and arrangements were made for fixing this roll on one side of the ship's compass, in the direction nearly transverse to the keel, and at the same height as the compass card.

The magnet being placed in the proper position and at the assigned distance below the compass, and the roll of iron plate being mounted, on making observations in the usual way, it was found (without waiting for the complete calculations) that the deviation was over-corrected. Whether any error had occurred in the measures, or whether the inductive action of the ship increased the power of the magnet (which was placed with its poles in the direction opposite to those of the ship) I do not know. The distance of the magnet was increased, its direction being carefully preserved, and then a series of observations was made and reduced in the usual manner. The results are contained in the following table.

No. of observation.	True azimuth of ship's head.	Disturbance of compass.	No. of observation.	True azimuth of ship's head.	Disturbance of compass.
	° ′	° ′		° ′	° ′
171	152 6	+0 54	177	334 22	−1 22
172	176 56	+0 34	178	357 34	−0 4
173	210 56	−0 41	179	26 34	−0 14
174	239 51	+0 9	180	56 6	+1 54
175	269 53	−0 8	181	87 51	+1 9
176	300 36	−0 6	182	118 36	+0 24

The correction may be considered as perfect as it is possible to make from observations of no greater delicacy than those on which the position of the correctors was determined. I think it is evident that the true maximum error does not exceed half a degree.

Proceeding in the same manner with Station II., we find the following equations.

$$\left(\frac{H}{I \cos \delta} + \tan \delta \,.\, N\right) + (-M + P) = -0{\cdot}32$$

$$\left(\frac{H}{I \cos \delta} + \tan \delta \,.\, N\right) = -0{\cdot}28$$

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correcting the ship's compass should be placed, the keepers were taken off, and the intensity of this magnet was found fully equal to that of its fellow. It was left during one night standing vertically with its marked end upwards: the next morning it had lost two-thirds of its power. I have since examined the powers of several pairs of magnets shortly after taking off their keepers and after the lapse of two or three days, and in every instance one magnet of each pair has lost much of its power.

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) - (-M + P) = -0{\cdot}34$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}32$$

$$\frac{S}{I\cos\delta} = -0{\cdot}21$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}14$$

$$\frac{S}{I\cos\delta} = -0{\cdot}22$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}27$$

The most probable values appear to be

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}32$$

$$-M + P = +0{\cdot}01$$

$$\frac{S}{I\cos\delta} = -0{\cdot}21$$

$$M + P = +0{\cdot}065$$

whence $M = 0{\cdot}03$, $P = 0{\cdot}04$. The last term denotes that the disturbance not depending on permanent magnetism would amount at its maximum to $2^\circ\frac{1}{4}$, changing signs in successive quadrants, and being negative (in its effects on apparent azimuth) when the azimuth of the ship's head is between 0 and 90°. The large terms show that the permanent magnetism (as compared with the terrestrial force) may be represented by 0·38, inclined to the direction of the keel by an angle α where $\tan\alpha = \frac{21}{32}$. Computing as for Station I., we get the following table of comparisons.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
	° ′	° ′	° ′	° ′		° ′	° ′	° ′	° ′
35	282 51	−21 36	−20 5	−1 31	45	103 36	+12 54	+11 50	+1 4
36	300 4	−12 41	−14 50	+2 9	46	124 26	+ 7 26	+ 6 10	+1 16
37	320 49	− 1 19	− 3 55	+2 36	47	143 57	+ 1 26	+ 1 0	+0 26
38	340 7	+ 9 31	+ 8 5	+1 26	48	163 16	− 4 16	− 4 30	+0 14
39	353 48	+15 4	+15 5	−0 1	49	179 6	− 9 6	− 8 50	−0 16
40	11 26	+18 34	+20 30	−1 56	50	203 1	−16 16	−14 45	−1 31
41	31 14	+19 46	+22 35	−2 49	51	222 21	−21 51	−18 55	−2 56
42	49 47	+19 51	+21 50	−1 59	52	244 56	−26 26	−22 5	−4 21
43	67 3	+18 12	+19 35	−1 23	53	267 46	−24 16	−22 30	−1 46
44	85 1	+14 59	+16 15	−1 16					

The residual term here follows with great accuracy the law of the deviation depending on P, and its magnitude is, as nearly as the nature of the observation allows, the same as that predicted.

For Station III.

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) + (-M + P) = -0{\cdot}18$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}26$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) - (-M + P) = -0{\cdot}32$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}31$$

$$\frac{S}{I\cos\delta} = -0{\cdot}18$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}14$$

$$\frac{S}{I\cos\delta} = -0{\cdot}17$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}28$$

The most probable values are

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}27$$

$$-M + P = +0{\cdot}07$$

$$\frac{S}{I\cos\delta} = -0{\cdot}19$$

whence

$$M + P = +0{\cdot}07$$

$$M = 0{\cdot}00, \qquad P = 0{\cdot}07.$$

The term of the disturbance of the compass which changes sign in the alternate quadrant ought therefore to have a maximum value of 4°; its sign, in each quadrant, ought to be the same as at Station I. and II. The permanent magnetism of the ship is represented by 0·33, inclined to the ship's keel at an angle whose natural tangent is $\frac{19}{27}$. On computing the effect of this independent magnetism, in the same manner as for Station I., the following Table is formed.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
54	284° 11′	−19° 41′	−16° 16′	−3° 31′	64	93° 11′	+11° 49′	+12° 30′	−0° 41′
55	300 24	−13 24	−11 30	−1 54	65	120 36	+ 7 54	+ 6 10	+1 44
56	311 18	− 4 18	− 6 45	+2 27	66	139 46	+ 3 44	+ 1 35	+2 9
57	324 26	+ 2 49	− 0 35	+3 24	67	163 9	− 3 9	− 4 30	+1 21
58	340 51	+ 7 39	+ 7 30	+0 9	68	187 9	−10 39	− 9 55	−0 44
59	2 34	+11 56	+15 15	−3 19	69	216 6	−18 21	−15 50	−2 31
60	22 16	+14 29	+18 40	−4 11	70	235 11	−21 41	−18 20	−3 21
61	38 11	+16 19	+19 25	−3 6	71	259 59	−24 29	−19 20	−5 9
62	60 34	+15 26	+17 55	−2 29	72	275 41	−21 41	−17 40	−4 1
63	81 36	+13 24	+14 40	−1 16					

Remarking that the residual errors with the sign − exceed in magnitude and number those with the sign +, I conjecture that the index error has been erroneously determined, to the extent perhaps of 40′ or 50′. With this supposition, the agreement of the observed residual term with the theoretical residual term is very close.

For Station IV. we have as follows:

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) + (-M + P) = -0{\cdot}11$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}14$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) - (-M + P) = -0{\cdot}18$$

$$\left(\frac{H}{I\cos\delta} + \tan\delta\,.\,N\right) = -0{\cdot}12$$

$$\frac{S}{I\cos\delta} = -0{\cdot}15$$

$$\frac{S}{I\cos\delta} + (M + P) = -0{\cdot}15$$

$$\frac{S}{I\cos\delta} = -0{\cdot}18$$

$$\frac{S}{I\cos\delta} - (M + P) = -0{\cdot}35$$

The most probable values appear to be

$$\frac{H}{I\cos\delta} + \tan\delta\,.\,N = -0{\cdot}14$$

$$-M + P = +0{\cdot}035$$

$$\frac{S}{I\cos\delta} = -0{\cdot}21$$

$$M + P = +0{\cdot}10$$

whence $M = 0{\cdot}03, \quad P = 0{\cdot}07.$

The coefficient therefore of the term which changes signs at alternate quadrants ought to be 4° nearly; the permanent magnetism ought to be represented by 0·25 inclined at an angle whose tangent $= \frac{21}{14}$. The following Table will show the comparison of the term computed (as before) from the permanent magnetism with the disturbance observed.

No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	No. of observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
	° ′	° ′	° ′	° ′		° ′	° ′	° ′	° ′
73	96 21	− 9 21	− 8 20	−1 1	83	310 6	+ 3 54	− 1 30	+5 24
74	111 6	− 2 6	− 4 5	+1 59	84	326 46	− 0 46	− 4 50	+4 4
76	150 36	+ 8 54	+ 8 35	+0 19	85	350 16	− 6 16	− 9 10	+2 54
77	167 6	+ 8 54	+12 15	−3 21	86	18 1	−12 31	−13 0	+0 29
78	183 41	+ 9 49	+14 20	−4 31	87	39 36	−15 21	−14 45	−0 36
79	196 46	+10 14	+14 40	−4 26	88	65 9	−15 39	−14 0	−1 39
80	229 54	+ 7 36	+12 50	−5 14	89	84 34	−12 4	−11 0	−1 4
81	246 59	+ 8 1	+10 20	−2 19					
82	275 6	+ 7 9	+ 5 50	+1 19					

The agreement of the residual term here with the theoretical residual term is not so perfect as at the other stations, but is nevertheless very close. A trifling alteration in the independent magnetism would make the agreement perfect. I may observe that, *à priori*, the existence of a disagreement at this station is probable. The observations of intensity and of disturbance of the compass were not made at the same time; and it is extremely probable that the box of chain-cable (mounted on wheels upon the deck, at no great distance from Station IV.) had been moved in the intermediate time.

Attempts were made to correct the compass at Stations II., III., and IV., in the same manner as at Station I. They did not, however, succeed so perfectly. I attribute this failure to the following cause. As the observations at Station I. had seemed to show that a small tentative adjustment might be necessary, a tentative adjustment was tried, depending of course upon the indications of the compass, at each of the other stations before making a round of observations. The compass it was found had become exceedingly sluggish (probably from the blunting of the suspending point by very frequent movements), two readings for the same bearing being sometimes obtained differing 8° or more. By one of these errors an adjustment might be affected, and its consequence would be found in every observation made while the magnets were in the position so adjusted. As a general rule it is to be recommended that no tentative adjustment be made, except the whole apparatus is in the highest possible order. When the vessel was prepared for sea, three compasses (at Stations I. II. and III.) were fitted with magnets and masses of soft iron placed according to the directions in the preceding section, and arranged, as to intensity and position of the magnets, and as to maximum effect of the soft iron, in accordance with the numbers just found; and the direction of these three compasses has appeared to be, as far as general observation could discover, quite correct. The compass at Station II. occasionally differed from the others two or three degrees; but this error might be occasioned by a blow which the magnet received, or might be produced by the effect of the powerful 2-feet magnet under Station I. The magnets under Stations II. and III. were 14-inch magnets, producing no sensible effect at any station but those to which they belonged.

On the whole, I conclude that the explanation of the deviations of the compass, by the combined powers of independent magnetism of the ship and induced magnetism produced by terrestrial action, is perfect; and that there is no reason to doubt that by the introduction of antagonist magnets and masses of soft iron the correction may be made perfect.

I shall now proceed with the discussion of the observations made with the dipping-needle.

The equation depending on the dip towards the ship's head given in the last section, being applied to Station I., and the substitution of 0·00 for $-M+P$, 0·80 for $\frac{H}{I\cos\delta}+\tan\delta\,.\,N$, and 72° 40′ for δ, being made, the equation assumes the following form:

$$0{\cdot}312\,.\cos A - 0{\cdot}250 = c\left(1+\frac{V}{I\sin\delta}-Q\right)+0{\cdot}312\,.\,c\,.\,N\,.\cos A.$$

Substituting in each of the observations from No. 35 to 53, for Station I., we have the following equations.

No. of observation.	Equation.
35	$-0{\cdot}181 = -0{\cdot}197\left(1+\frac{V}{I\sin\delta}-Q\right)-{\cdot}014\,N.$
37	$-0{\cdot}008 = -0{\cdot}047$ ..... $-{\cdot}011$ ..
38	$+0{\cdot}043 = +0{\cdot}010$ ..... $+{\cdot}003$ ..
39	$+0{\cdot}060 = +0{\cdot}032$ ..... $+{\cdot}010$ ..
40	$+0{\cdot}056 = +0{\cdot}033$ ..... $+{\cdot}010$ ..
41	$+0{\cdot}017 = -0{\cdot}015$ ..... $-{\cdot}004$ ..
42	$-0{\cdot}048 = -0{\cdot}067$ ..... $-{\cdot}013$ ..
43	$-0{\cdot}128 = -0{\cdot}132$ ..... $-{\cdot}016$ ..
44	$-0{\cdot}223 = -0{\cdot}206$ ..... $-{\cdot}005$ ..
45	$-0{\cdot}323 = -0{\cdot}320$ ..... $+{\cdot}023$ ..
46	$-0{\cdot}427 = -0{\cdot}404$ ..... $+{\cdot}071$ ..
47	$-0{\cdot}502 = -0{\cdot}468$ ..... $+{\cdot}118$ ..
48	$-0{\cdot}548 = -0{\cdot}535$ ..... $+{\cdot}160$ ..
49	$-0{\cdot}562 = -0{\cdot}552$ ..... $+{\cdot}172$ ..
50	$-0{\cdot}537 = -0{\cdot}535$ ..... $+{\cdot}153$ ..
51	$-0{\cdot}480 = -0{\cdot}473$ ..... $+{\cdot}109$ ..
52	$-0{\cdot}382 = -0{\cdot}392$ ..... $+{\cdot}052$ ..
53	$-0{\cdot}262 = -0{\cdot}285$ ..... $+{\cdot}003$ ..

The sum of the equations from 35 to 44, and from 45 to 53, give respectively,

$$-0{\cdot}412 = -0{\cdot}589\left(1+\frac{V}{I\sin\delta}-Q\right)-0{\cdot}030\,.\,N.$$

$$-4{\cdot}023 = -3{\cdot}964\left(1+\frac{V}{I\sin\delta}-Q\right)+0{\cdot}861\,.\,N.$$

When the rudeness of the observations is considered, it will be evident that the first of these equations is wholly unfit (from the smallness of its coefficients) to be combined with the latter for the determination of two unknown quantities. If we add them together we find

$$-4{\cdot}435 = -4{\cdot}553\left(1+\frac{V}{I\sin\delta}-Q\right)+0{\cdot}831\,.\,N$$

and if we assume N to be small (which assumption appears, from the nature of the integral on which it depends, to be well founded), we must infer that the vertical part of the ship's permanent magnetism is very small.

The equation for the transversal dip, substituting 0·04 for M + P and − 0·17 for $\frac{S}{I \cos \delta}$, becomes

$$-0{\cdot}300 \,.\, \sin A - 0{\cdot}053 = c' \left(1 + \frac{V}{I \sin \delta} - Q\right) + 0{\cdot}312 \,.\, c' \,.\, N \,.\, \cos A.$$

Substituting in each of the observations from 54 to 72, we get the following equations.

No. of observation.	Equation.	
54	$+ {\cdot}238 = + {\cdot}203 \left(1 + \frac{V}{I \sin \delta} - Q\right)$	$+ {\cdot}015$ N.
55	$+ {\cdot}217 = + {\cdot}190$	$+ {\cdot}024$ ..
56	$+ {\cdot}172 = + {\cdot}114$	$+ {\cdot}024$ ..
57	$+ {\cdot}121 = + {\cdot}079$	$+ {\cdot}020$ ..
58	$+ {\cdot}045 = + {\cdot}016$	$+ {\cdot}005$ ..
59	$- {\cdot}068 = - {\cdot}070$	$- {\cdot}022$ ..
60	$- {\cdot}167 = - {\cdot}176$	$- {\cdot}051$ ..
61	$- {\cdot}238 = - {\cdot}245$	$- {\cdot}060$ ..
62	$- {\cdot}314 = - {\cdot}312$	$- {\cdot}048$ ..
63	$- {\cdot}350 = - {\cdot}381$	$- {\cdot}017$ ..
64	$- {\cdot}352 = - {\cdot}344$	$+ {\cdot}006$ ..
65	$- {\cdot}311 = - {\cdot}315$	$+ {\cdot}050$ ..
66	$- {\cdot}247 = - {\cdot}249$	$+ {\cdot}059$ ..
67	$- {\cdot}140 = - {\cdot}196$	$+ {\cdot}058$ ..
68	$- {\cdot}016 = - {\cdot}041$	$+ {\cdot}013$ ..
69	$+ {\cdot}126 = + {\cdot}087$	$- {\cdot}022$ ..
70	$+ {\cdot}193 = + {\cdot}148$	$- {\cdot}026$ ..
71	$+ {\cdot}243 = + {\cdot}196$	$- {\cdot}011$ ..
72	$+ {\cdot}246 = + {\cdot}203$	$- {\cdot}006$ ..

The sum of the equations from 54 to 58 gives

$$(1) + {\cdot}793 = + {\cdot}602 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}088 \,.\, N.$$

The sum of the equations from 59 to 63 gives

$$(2) - 1{\cdot}137 = - 1{\cdot}184 \left(1 + \frac{V}{I \sin \delta} - Q\right) - {\cdot}198 \,.\, N.$$

The sum of the equations from 64 to 68 gives

$$(3) - 1{\cdot}066 = - 1{\cdot}145 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}186 \, N.$$

The sum of the equations from 69 to 72 gives

$$(4) + {\cdot}808 = + {\cdot}634 \left(1 + \frac{V}{I \sin \delta} - Q\right) - {\cdot}065 \,.\, N.$$

(1) − (2) − (3) + (4) give

$$+ 3{\cdot}804 = + 3{\cdot}565 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}035 \, N.$$

(1) − (2) + (3) − (4) give

$$+ 0{\cdot}056 = + {\cdot}007 \left(1 + \frac{V}{I \sin \delta} - Q\right) + {\cdot}537\, N.$$

From the two last equations, $1 + \frac{V}{I \sin \delta} - Q = 1{\cdot}06$, $N = + {\cdot}09$; which values, with very trifling alterations, would satisfy the equation derived from the longitudinal dips. The excess of the numbers on the first side with the sign + and the defect of those with the sign − might be caused by an erroneous determination of $\frac{S}{I \cos \delta}$, but it is far more probable that it has originated here in an error of adjustment of the dipping-needle, made while the ship was heeling: it produces no sensible effect on the solutions of the equations.

The equation for dip towards the ship's head at Station III. becomes, on substituting the numbers proper for that station,

$$0{\cdot}407 \cos A - 0{\cdot}103 = c \left(1 + \frac{V}{I \sin \delta} - Q\right) + c\,.\,0{\cdot}381 \cos A\,.\,N.$$

Forming the separate equations, combining those from No. 35 to 43 in one group, and those from 44 to 53 in another group, we obtain

$$+ 1{\cdot}629 = + 1{\cdot}855 \left(1 + \frac{V}{I \sin \delta} - Q\right) + 0{\cdot}591\,.\,N$$

$$+ 3{\cdot}308 = - 3{\cdot}260 \left(1 + \frac{V}{I \sin \delta} - Q\right) + 0{\cdot}924\,.\,N.$$

From these, $1 + \frac{V}{I \sin \delta} - Q = 0{\cdot}95$, $N = - 0{\cdot}22$.

The equation for dip towards the starboard side at the same station is

$$- 0{\cdot}354 \sin A - {\cdot}072 = c' \left(1 + \frac{V}{I \sin \delta} - Q\right) + c'\,.\,0{\cdot}381 \cos A\,.\,N.$$

Grouping the equations from 54 to 57, 60 to 63, 64 to 68, 69 to 71, and 72, and then combining these groups so as to make all the multipliers of the unknown quantities additive, one in each equation, we get the following equations

$$+ 4{\cdot}357 = + 4{\cdot}416 \left(1 + \frac{V}{I \sin \delta} - Q\right) + 0{\cdot}081\,.\,N.$$

$$+ 0{\cdot}485 = + 0{\cdot}524 \left(1 + \frac{V}{I \sin \delta} - Q\right) + 0{\cdot}777\,.\,N.$$

From these, $1 + \frac{V}{I \sin \delta} - Q = 0{\cdot}99$, $N = - 0{\cdot}04$.

The agreement of these results with those obtained for the dip towards the head is as close as could be expected. The equations for determining N cannot be made very favourable, and great precision in its value is not to be hoped for. The agreement of the two values of $1 + \frac{V}{I \sin \delta} - Q$ is a proof of the accuracy of the determinations of horizontal intensity, and of the general correctness of the theory which

connects the different observations. For upon the values of P, deduced from the observations of intensity, the first term in every one of these equations depends (the difference in the numerical values of the coefficients 0·407 and 0·354 depends entirely upon it): a very small alteration of P would make the results for $1 + \frac{V}{I \sin \delta} - Q$ precisely equal: the omission of P would have caused very great discordance in the values of $1 + \frac{V}{I \sin \delta} - Q$.

I have not been equally successful with the observations at Station IV. The dipping-needle used there was, as I have mentioned, in the proximity of large moveable masses of iron, the windlass, the anchors, and the box of chain-cable. Whatever the cause may be, the observations are in themselves discordant. Upon trying whether the cotangent of the dip could be approximately represented by $a \cos A + b$ for dips towards the head, and by $a' \sin A + b'$ for dips towards the starboard side, it was found that the discordances were between three and four times as great at Station IV. as at Station III. I have not thought it worth while to occupy space with the equations, &c. at this station; considering that the results already obtained are sufficient for my present purpose.

The quantity Q it will be remarked (from its expression in Section III.) is of the same order as M, and therefore probably small. Hence the assertion that $1 + \frac{V}{I \sin \delta} - Q$ differs little from 1, enables us at once to assert that V, the permanent vertical magnetic force of the ship, is small.

Section VI.—*Observations not essential to the Theory.*

The following account of the observed disturbance of the compass on the upper floor of the stage will probably be sufficient.

At Station I. the disturbance vanished when the azimuth of the ship's head was about 150° and 320°. The greatest + disturbance (needle deviating to the left) took place in azimuth 78° nearly, and amounted to + 5°; the greatest − disturbance occurred in azimuth 260°, and amounted to − 10°.

At Station II. the disturbance vanished in azimuths 156° and 330° nearly. The greatest + disturbance was in azimuth 50° or 55°, and exceeded 10°; the greatest − disturbance was in azimuth 267°, and amounted to $11\frac{1}{2}$°.

At Station III. the disturbance vanished in azimuth 150° and 330° nearly. The greatest + disturbance was in azimuth 76°, and amounted to $7\frac{1}{4}$°; the greatest − disturbance was in azimuth 230°, and amounted to $9\frac{1}{2}$°.

At Station IV. the disturbance vanished in azimuths 155° and 320° nearly. The greatest + disturbance was in azimuth 60° nearly, and amounted to 8°; the greatest − disturbance was in azimuth 230° nearly, and amounted to about 8°.

These disturbances are evidently referable, for the greatest part of their amount,

to permanent magnetism; though the effect of induced magnetism is sensible, especially in the observations at Station I.

The observations made with the dipping-needles and large horizontal magnets on shore, were conducted under circumstances of weather so unfavourable (amidst torrents of rain and very heavy gusts of wind), that it has not appeared worth while to transcribe them. The following result, however, may be stated as deducible from them with perfect certainty. On one or two occasions there appeared to be reason to think that the ship's head, when near, attracted the upper end of the dipping-needle, or the unmarked end of the horizontal magnet, but the effect hardly exceeded the errors of observation. But in every instance when the ship's stern was brought near to the dipping-needles or magnets, it powerfully attracted the lower or marked end. The most distinct estimate of its power may be obtained from the following observation. On August 2, the ship's head being south nearly, and a dipping-needle vibrating in the meridian being placed at the distance 25 feet from the stern, the dip of the needle was increased 7° 30′. I may remark that the dipping-needle is an infinitely less delicate instrument (where horizontal forces only are concerned) than the large horizontal magnet, as used by GAUSS, with reflector and graduated scale, suspended by one bundle of silk fibres if parallel to the magnetic meridian, or by two if transverse to it. The dipping-needle, however, is mounted with much greater speed and less trouble than the magnet, and is more easily protected from the violence of the weather.

Section VII.—*Observations of the Disturbance of the Compass in the Iron-built Sailing-ship Ironsides.*

On the 26th and 27th of October 1838, I examined the binnacle compass of the iron sailing-ship Ironsides in the Brunswick Dock, Liverpool. The observations were made by placing one azimuth compass in the position of the binnacle, and another on shore, and with each of these compasses observing a small signal carried by the other. In this manner the reductions for locality, &c. were completely avoided, but every result was liable to the errors of two compasses. The experience of the investigations in the Rainbow having shown the importance of observations of horizontal intensity, I applied the intensity apparatus in every position of the ship in which the compass was observed. The time occupied by forty vibrations of the needle on shore was 195ˢ·0. The values for disturbing forces in the following Table are computed by the methods of Section IV.

Reference to observation.	Azimuth of ship's head.	Disturbance of compass.	Time of forty vibrations.	Intensity to N. / I cos δ	Dist. force to N. / I cos δ	Dist. force to E. / I cos δ	Dist. force to head. / I cos δ	Dist. force to starboard. / I cos δ
	° ′	° ′	s					
a	0 50	−28 20	262·8	0·485	−0·515	+0·261	−0·462	+0·345
b	23 0	−22 30	279·4	0·450	−0·550	+0·186	−0·433	+0·387
c	32 35	−11 20	299·2	0·416	−0·584	+0·083	−0·447	+0·385
d	47 40	+ 6 40	303·3	0·411	−0·589	−0·048	−0·432	+0·403
e	58 55	+16 50	289·4	0·435	−0·565	−0·131	−0·405	+0·416
f	78 40	+28 50	256·3	0·507	−0·493	−0·279	−0·371	+0·428
g	89 0	+35 0	228·4	0·597	−0·403	−0·418	−0·425	+0·396
h	112 0	+37 10	204·4	0·810	−0·190	−0·416	−0·314	+0·332
i	151 0	+25 30	179·8	1·062	+0·062	−0·506	−0·299	+0·413
k	171 50	+16 0	171·4	1·244	+0·244	−0·357	−0·293	+0·318
l	184 55	+10 55	166·1	1·353	+0·353	−0·261	−0·330	+0·290
m	211 30	− 0 40	163·0	1·431	+0·431	+0·017	−0·376	+0·211
n	233 30	− 6 20	163·2	1·419	+0·419	+0·157	−0·376	+0·243
o	250 35	−11 50	166·2	1·347	+0·347	+0·282	−0·381	+0·233
p	279 35	−20 20	174·4	1·172	+0·172	+0·434	−0·399	+0·242
q	304 20	−25 20	188·7	0·965	−0·035	+0·457	−0·397	+0·229
r	333 10	−30 30	209·7	0·745	−0·255	+0·439	−0·426	+0·277
s	350 50	−29 20	235·6	0·597	−0·403	+0·336	−0·452	+0·267

Expressing each of the values of $\frac{\text{Disturbing force to head}}{\text{I} \cos \delta}$ by the formula

$$\left(\frac{\text{H}}{\text{I} \cos \delta} + \tan \delta \,.\, \text{N}\right) + (\text{P} - \text{M}) \cos \text{A},$$

we get the following series of equations.

Reference to observation.	Equation.
a	$-0{\cdot}462 = \left(\frac{\text{H}}{\text{I} \cos \delta} + \tan \delta \,.\, \text{N}\right) + 0{\cdot}985\,(\text{P} - \text{M})$
b	$-0{\cdot}433 = \ldots\ldots\ldots\ldots + 0{\cdot}921 \ldots\ldots$
c	$-0{\cdot}447 = \ldots\ldots\ldots\ldots + 0{\cdot}843 \ldots\ldots$
d	$-0{\cdot}432 = \ldots\ldots\ldots\ldots + 0{\cdot}673 \ldots\ldots$
e	$-0{\cdot}405 = \ldots\ldots\ldots\ldots + 0{\cdot}516 \ldots\ldots$
f	$-0{\cdot}371 = \ldots\ldots\ldots\ldots + 0{\cdot}197 \ldots\ldots$
g	$-0{\cdot}425 = \ldots\ldots\ldots\ldots + 0{\cdot}017 \ldots\ldots$
h	$-0{\cdot}314 = \ldots\ldots\ldots\ldots - 0{\cdot}375 \ldots\ldots$
i	$-0{\cdot}299 = \ldots\ldots\ldots\ldots - 0{\cdot}875 \ldots\ldots$
k	$-0{\cdot}293 = \ldots\ldots\ldots\ldots - 0{\cdot}990 \ldots\ldots$
l	$-0{\cdot}330 = \ldots\ldots\ldots\ldots - 0{\cdot}996 \ldots\ldots$
m	$-0{\cdot}376 = \ldots\ldots\ldots\ldots - 0{\cdot}853 \ldots\ldots$
n	$-0{\cdot}376 = \ldots\ldots\ldots\ldots - 0{\cdot}595 \ldots\ldots$
o	$-0{\cdot}381 = \ldots\ldots\ldots\ldots - 0{\cdot}332 \ldots\ldots$
p	$-0{\cdot}399 = \ldots\ldots\ldots\ldots + 0{\cdot}166 \ldots\ldots$
q	$-0{\cdot}397 = \ldots\ldots\ldots\ldots + 0{\cdot}564 \ldots\ldots$
r	$-0{\cdot}426 = \ldots\ldots\ldots\ldots + 0{\cdot}892 \ldots\ldots$
s	$-0{\cdot}452 = \ldots\ldots\ldots\ldots + 0{\cdot}987 \ldots\ldots$

The sum of all the equations gives

$$-7{\cdot}018 = 18\left(\frac{\text{H}}{\text{I} \cos \delta} + \tan \delta \,.\, \text{N}\right) + 1{\cdot}745 \,.\, (\text{P} - \text{M}).$$

The sum of all, changing the signs of those from *g* to *p*, gives

$$-0{\cdot}632 = +11{\cdot}411 \,.\, (\text{P} - \text{M}).$$

From these,

$$\text{P} - \text{M} = -0{\cdot}055, \quad \frac{\text{H}}{\text{I} \cos \delta} + \tan \delta \,.\, \text{N} = -0{\cdot}386.$$

Again, expressing each of the values of $\frac{\text{disturbing force to starboard}}{\text{I} \cos \delta}$ by the formula

$$\frac{\text{S}}{\text{I} \cos \delta} + (\text{P} + \text{M}) \sin \text{A},$$

we have the following equations.

Reference to observation.	Equation.
a	$+ 0{\cdot}345 = \frac{\text{S}}{\text{I} \cos \delta} + 0{\cdot}171 \,.\, (\text{P} + \text{M})$
b	$+ 0{\cdot}387 = \ldots\ldots + 0{\cdot}391 \ldots\ldots$
c	$+ 0{\cdot}385 = \ldots\ldots + 0{\cdot}539 \ldots\ldots$
d	$+ 0{\cdot}403 = \ldots\ldots + 0{\cdot}739 \ldots\ldots$
e	$+ 0{\cdot}416 = \ldots\ldots + 0{\cdot}856 \ldots\ldots$
f	$+ 0{\cdot}428 = \ldots\ldots + 0{\cdot}981 \ldots\ldots$
g	$+ 0{\cdot}396 = \ldots\ldots + 1{\cdot}000 \ldots\ldots$
h	$+ 0{\cdot}332 = \ldots\ldots + 0{\cdot}927 \ldots\ldots$
i	$+ 0{\cdot}413 = \ldots\ldots + 0{\cdot}485 \ldots\ldots$
k	$+ 0{\cdot}318 = \ldots\ldots + 0{\cdot}142 \ldots\ldots$
l	$+ 0{\cdot}290 = \ldots\ldots - 0{\cdot}086 \ldots\ldots$
m	$+ 0{\cdot}211 = \ldots\ldots - 0{\cdot}523 \ldots\ldots$
n	$+ 0{\cdot}243 = \ldots\ldots - 0{\cdot}804 \ldots\ldots$
o	$+ 0{\cdot}233 = \ldots\ldots - 0{\cdot}943 \ldots\ldots$
p	$+ 0{\cdot}242 = \ldots\ldots - 0{\cdot}986 \ldots\ldots$
q	$+ 0{\cdot}229 = \ldots\ldots - 0{\cdot}826 \ldots\ldots$
r	$+ 0{\cdot}277 = \ldots\ldots - 0{\cdot}451 \ldots\ldots$
s	$+ 0{\cdot}267 = \ldots\ldots - 0{\cdot}159 \ldots\ldots$

The sum of all the equations gives

$$+ 5{\cdot}815 = 18 \cdot \frac{\text{S}}{\text{I} \cos \delta} + 1{\cdot}453 \,.\, (\text{P} + \text{M}).$$

The sum of all, changing the signs from *k* to *s*, gives

$$+ 1{\cdot}195 = + 10{\cdot}725 \, (\text{P} + \text{M}).$$

From these,

$$\text{P} + \text{M} = + 0{\cdot}111, \quad \frac{\text{S}}{\text{I} \cos \delta} = + 0{\cdot}314.$$

and

$$\text{M} = 0{\cdot}083, \quad \text{P} = 0{\cdot}028.$$

The permanent magnetic force may therefore be represented by the force

$$\sqrt{\{0{\cdot}\overline{386}^2 + 0{\cdot}\overline{314}^2\}} = 0{\cdot}50,$$

inclined to the ship's keel at an angle whose tangent $= \frac{-314}{386}$. In compounding this, for the different positions of the ship, with the terrestrial force, it is to be observed that the diminution 0·08 is too serious to be neglected (as we have hitherto done), and that 0·92 must therefore be considered as measuring the terrestrial force. With these numbers the effect of the permanent magnetism is computed graphically as before; and the result, and its comparison with the observed disturbance, are contained in the following Table.

Reference to observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.	Reference to observation.	Azimuth of ship's head.	Observed disturbance of compass.	Computed disturbance of compass.	Disturbance yet to be accounted for.
	° ′	° ′	° ′	° ′		° ′	° ′	° ′	° ′
a	9 50	−28 20	−27 20	−1 0	*k*	171 50	+16 0	+16 10	−0 10
b	23 0	−22 30	−18 0	−4 30	*l*	184 55	+10 55	+11 45	−0 50
c	32 35	−11 20	− 7 30	−3 50	*m*	211 30	− 0 40	+ 2 30	−3 10
d	47 40	+ 6 40	+10 0	−3 20	*n*	233 30	− 6 20	− 5 5	−1 15
e	58 55	+16 50	+20 45	−3 55	*o*	250 35	−11 50	−11 0	−0 50
f	78 40	+28 50	+30 45	−1 55	*p*	279 35	−20 20	−20 30	+0 10
g	89 0	+35 0	+32 25	+2 35	*q*	304 20	−25 20	−27 20	+2 0
h	112 0	+27 10	+31 30	−4 20	*r*	333 10	−30 30	−32 30	+2 0
i	151 0	+25 30	+22 30	+3 0	*s*	350 50	−29 20	−30 0	+0 40

From the value of P above, the residual term ought to have for maximum value 1°·6, supposing the permanent magnetism not to act at the same time; in simultaneous action the maximum would be nearly doubled in the first quadrant (from the weakening of the terrestrial force by the permanent magnetism), and would be diminished by one-third in the third quadrant; the signs would be − + − + in the four quadrants. The agreement of the signs and magnitudes of the residual terms with those defined by this law is extremely close; the value for observation *h* alone presenting a sensible disagreement. The theory, therefore, may be considered as perfectly in accordance with the facts observed in the deviations and intensities at the position of this compass.

The maximum of the term depending on P being (when the permanent magnetism is counteracted) 1°·6, it was thought unnecessary to encumber the binnacle with a mass of iron for the correction of that small quantity. The only correction applied was a magnet, placed below the compass, in a position making the angle whose tangent is $\frac{-314}{386}$ with the ship's keel, and at a distance at which its effect (as found by trial on shore) was 0·50 of the earth's directive force, or produced a deviation of 27° nearly when transverse to the meridian. The deviation of the compass was then found to be as follows.

Azimuth of ship's head.	Disturbance of compass.
° ′	° ′
3 15	+1 30
45 45	−1 15
94 45	−1 45
182 45	−0 45
224 0	−2 30
276 20	+0 40
323 0	−2 30

The whole of these apparent disturbances are within the limits of errors of observation; it is difficult to say whether they contain any distinct trace of the inductive term.

Another compass in the same ship (a tell-tale, or compass suspended to a beam

in the cabin) was observed in regard to deviation only. The observations were made by an incompetent person, and are not worth transcribing. The maximum deviation was greater than that at the binnacle. But this singular circumstance presented itself: that the deviation vanished when the azimuth of the ship's head was 140° and 320° nearly, the maximum + error occurring near azimuth 200°, and the maximum − error near azimuth 80°. At the binnacle compass, the deviation vanished in azimuths 40° and 220° nearly, and its maximum + and − errors occurred in azimuths 90° and 340° nearly. Therefore, to make the direction of the ship's independent magnetism at the tell-tale parallel to the magnetic meridian, it was necessary to turn the ship 100 degrees further than was necessary to effect the same for the binnacle compass. Or, supposing the head of the ship towards the top of the page, the direction of the magnetic force (as acting on the marked end of the needle) is ↘ at the binnacle, and ↗ at the tell-tale. These stations are not, if I remember right, more than twelve feet apart. In the Rainbow, the directions of the forces on the four compasses were represented by lines as ↓ or ↙, all included within a small portion of the same quadrant.

The correction of this compass (the tell-tale) was effected by a tentative method, which is likely to be of the highest value in the correction of the compasses of iron ships in general. The ship's head being placed exactly north, as ascertained by a shore compass, a magnet was placed upon the beam from which the compass was suspended, with the direction of its length exactly transverse to the ship's keel: it was moved upon the beam to various distances till the compass pointed correctly, and then it was fixed. Then the ship's head was placed exactly east; and another magnet with its length parallel to the ship's keel was placed upon the same beam, and moved to different distances till the compass pointed correctly, and then it was fixed. The correction for induced magnetism was neglected: but there would have been no difficulty in adjusting it by the same process, placing the vessel's head in azimuth 45°, or 135°, or 225°, or 315°. The peculiar advantage in the method above given, as a tentative method, consists in this: that the magnetic action is resolved into two parts, either of which can be altered without at all disturbing the other, and that by a very simple rule those positions of the ship can be found in which each of these parts is effective while the other is powerless. The same remark applies to the correction for induced magnetism.

The Ironsides has since that time sailed to Pernambuco, and her compasses have been correct (as far as general observation goes) through the voyage. It is probable, therefore, that the constant N is not very large: but no accurate observations have been yet reported to me upon which any certain statement as to that point can be founded.

Section VIII.—*Concluding Remarks.*

It appears from the investigations above, that the deviations of the compass at four stations in the Rainbow, and at two stations in the Ironsides, are undoubtedly caused by two modifications of magnetic power; namely, the independent magnetism of the ship, which retains the same magnitude and the same direction relatively to the ship in all positions of the ship; and the induced magnetism, whose force varies in magnitude and direction while the ship's position is changed. It appears also that, in the instances mentioned, the effect of the former force greatly exceeds that of the latter.

It appears also that experiments and investigations similar to those applied above, are sufficient to obtain with accuracy the constants on which, at any one place, the ship's action upon the horizontal needle depends (namely, $\frac{H}{I \cos \delta} + \tan \delta \,.\, N$, $\frac{S}{I \cos \delta}$, M, and P); and that by placing a magnet so that its action will take place in a direction opposite to that which the investigations show to be the direction of the ship's independent magnetic action, and at such a distance that its effect is equal to that of the ship's independent magnetism, and by counteracting the effect of the induced magnetism by means of the induced magnetism of another mass (according to rules which are given), the compass may be made to point exactly as if it were free from disturbance.

It appears also that by an easy tentative method, the compass may now be corrected without the labour of any numerical investigations, or any experiments, except those of merely making the trials.

It appears also that the permanent vertical disturbing force, as far as the examination of the Rainbow authorizes us to draw any distinct conclusion, is not great, and therefore that there is no fear of great disturbance of the compass by the heeling of the ship.

But it appears that one of the magnetic constants consists of two parts, which cannot be separated by experiments on the horizontal magnet at any one place; and that the effect of the impracticability of separating these parts will be, to render the compass incorrect in one magnetic latitude when it has been made correct in a different magnetic latitude (though there is good reason to think that the term on which the variation depends is so small that it may be neglected, except in the case of a ship sailing very near the magnetic pole). And it appears that though in theory the term in question could be determined from observations of the dipping-needle, yet in practice the method fails, because the observations cannot be made with the requisite accuracy. It appears, however, that this term may be determined by observations of the horizontal needle at two places whose magnetic latitudes are different, and that the correction may then be made perfect for all magnetic latitudes.

To these considerations we may add the following: that though the uniformity of

the induced magnetism, under similar circumstances, is to be presumed, yet the invariability of the independent magnetism during a course of many years is by no means certain.

These statements suggest the following as rules which it is desirable to pursue in the present infancy of iron-ship building.

1. It appears desirable that every iron sea-going ship should be examined by a competent person, for the accurate determination of the four constants,

$$\frac{H}{I \cos \delta} + \tan \delta \,.\, N, \quad \frac{S}{I \cos \delta}, \quad M, \quad \text{and } P,$$

for each of the compasses in the ship; and that a careful record of these determinations should be preserved, as a magnetic register of the ship.

2. It appears desirable that the same person should examine the vessel at different times, with the view of ascertaining whether either of the constants changes with time.

3. It appears desirable that in the case of vessels going to different magnetic latitudes, the same person should arrange for the examination of the compasses in other places, with a view to the determination of N.

4. It appears desirable that the same person should examine and register the general construction of the ship, the position and circumstances of her building, &c., with the view of ascertaining how far the values of the magnetic constants depend on these circumstances, and in particular to ascertain their connexion with the value of the prejudicial constant M.

5. It appears desirable that the same person should see to the proper application of the correctors, and the proper measures for preserving the permanency* of their magnetism.

The most remarkable result, in a scientific view, from the experiments detailed above, is the great intensity of the permanent magnetism of the malleable iron of which the ship is composed. It appears, however, that almost every plate of rolled iron is intensely magnetic. In the progress of the investigations I have endeavoured to make some experiments with plates of iron, for the purpose of examining the induced magnetism of iron plates in different states of connexion, but they have failed totally, in consequence of the amount of independent magnetism. On placing a compass within a large cylinder of iron plate, whose axis was horizontal and in the magnetic meridian, I was surprised to see that the deviation of the compass was as great as at the binnacle position in the Rainbow.

The scrolls of iron used for correcting the induced magnetism in the Rainbow were repeatedly passed through the fire before their permanent magnetism was destroyed. In some which were tried, even this process was ineffectual. I am disposed to prefer

* The magnets which I have fixed in the Rainbow and the Ironsides have been heated to a temperature of 120° Fahr., with their marked ends towards the south, and have afterwards stood vertically with their marked ends upwards for one or two days. For mounting in the ship they have been let into grooves in pieces of wood, in which they have been bedded in tallow.

for correctors boxes filled with iron chain; for though many parts of the chain may possess some independent magnetism, yet it is likely that there will be, to a great extent, an intermutual destruction of effects.

If we conceive permanent magnetism to depend upon an artificial arrangement of the particles of the metal, the manufacture of rolled iron seems to account in some degree for this amount of magnetism. The iron, after leaving the puddling furnace, is rolled out into bars of considerable length; these are broken, and their pieces are laid side by side, and the united pieces are again rolled out, &c. The whole object of the manufacture is to arrange the particles in that artificial state known by the name *fibre*.

It is believed by practical men that the state of malleable iron changes from time only. If this be certain, and if the notion just mentioned be plausible, it seems sufficiently probable that the independent magnetism of the ship will change with time. This consideration enforces strongly the necessity of periodical examination as suggested above. Such examination may possibly have an advantage beyond the correction of the compass for the time. An important change in the magnetism may indicate an important alteration in the quality of the iron, and may serve as a warning to be cautious of trusting to the strength of the ship in critical circumstances.

I know not whether the axis of magnetism in a plate of iron may be expected probably to coincide with the direction in which it has been extended by rolling. The direction of the horizontal magnetism in the Rainbow (conceiving the transverse part to be produced by the transversal partitions or bulk heads), and the insignificant amount of the vertical magnetism, seem to countenance this supposition. The great difference between the magnetism of the two compasses in the Ironsides may seem, perhaps, to place difficulties in the way of such a supposition.

Royal Observatory, Greenwich,
April 9, 1839.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above the plinth at Waterloo Bridge....83 feet 2 inches.
_____ above the mean level of the sea .....97 feet.
Height of the receiver of the Rain Gauge above the court of Somerset House ..79 feet.
The External Thermometer is 2 feet higher than the Barometer Cistern.
The Thermometers are graduated to Fahrenheit's scale.
The Barometer is divided into inches and tenths.
The Hours of Observation are of Mean Time, the day beginning at Midnight.
The *daily* observations of the Barometer are *not* corrected.
The *monthly means* are corrected for capillarity and temperature by the Table contained in Mr. Baily's paper in *Phil. Trans.* for 1837.

METEOROLOGICAL JOURNAL FOR JULY AND AUGUST, 1838.

1838.	9 o'clock, A.M. Barometer uncorrected. Flint Glass	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering Lowest	External Thermometers. Self-registering Highest	Rain in Inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JULY															
⊙ 1	29.928	29.922	64.7	29.956	29.950	64.8	57	06.9	62.2	64.3	55.5	67.0	.055	SE	Lightly overcast throughout the day. Evening, Light steady rain.
M 2	30.048	30.042	67.4	30.044	30.040	66.8	59	01.6	61.8	70.4	57.3	65.7	.500	E	Cloudy—high wind throughout the day. Ev. Overcast—light rain.
T 3	30.070	30.062	65.2	30.080	30.074	65.0	60	05.1	63.6	62.2	59.7	71.4	.050	NW	Overcast—light steady rain nearly the whole of the day.
W 4	30.140	30.134	65.6	30.142	30.138	66.2	59	06.8	65.5	69.4	58.2	68.0	.352	S	A.M. Overcast—light rain and wind. P.M. Fine—light clouds. Evening, Fine and clear.
T 5	30.198	30.194	75.0	30.164	30.158	69.4	60	09.6	67.8	73.8	57.6	71.4	.050	E	Fine—nearly cloudless—lt. wind throughout the day. Ev. Fine & clear.
F 6	30.050	30.044	67.6	29.990	29.982	69.3	63	02.9	64.0	73.3	60.2	74.0	.244	SE	A.M. Overcast—thunder and lightning, with heavy rain and wind. P.M. Fine—light clouds. Evening, Cloudy—light wind.
○ S 7	30.010	30.002	69.3	29.982	29.974	67.4	59	07.6	64.3	66.4	56.7	73.2	.055	S	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—light rain and wind. Evening, Overcast.
⊙ 8	29.968	29.960	65.7	30.004	30.000	67.6	59	06.4	62.7	70.4	58.0	69.6	.088	SW var.	Overcast—light brisk wind throughout the day.
M 9	30.182	30.178	70.2	30.166	30.160	69.7	60	08.3	66.2	74.2	59.2	70.6		S	Fine—light clouds & wind throughout the day. Ev. Fine and clear.
T 10	30.204	30.198	67.8	30.178	30.172	70.7	61	06.5	65.2	73.8	59.2	74.6		SW	A.M. Lightly overcast—light wind. P.M. Cloudy—light brisk wind. Evening, Cloudy.
W 11	30.160	30.154	71.7	30.130	30.124	71.9	61	08.0	69.6	76.6	59.3	74.3		S	Fine—light clouds and wind throughout the day.
T 12	30.100	30.094	69.3	30.086	30.080	71.7	63	07.1	67.4	73.2	61.4	78.2		S var.	A.M. Overcast—light brisk wind. P.M. Cloudy—brisk wind. Evening, Fine & clear.
F 13	30.030	30.024	75.7	29.896	29.892	71.9	62	08.7	69.7	78.3	60.7	73.6		W	A.M. Cloudy—lt. breeze. P.M. Fine—lt. clds. Ev. Overcast—h. wind.
S 14	29.766	29.760	70.6	29.752	29.748	70.8	63	06.4	66.3	64.7	62.5	78.8		SW	A.M. Overcast—very high wind. P.M. Lt. rain & wind. Ev. The same.
⊙ 15	29.762	29.756	72.6	29.844	29.838	69.7	60	07.9	65.7	63.8	59.0	68.0	.183	SW var.	A.M. Cloudy—light wind. P.M. Overcast—light rain. Ev. Cloudy.
M 16	30.096	30.092	77.8	30.156	30.150	70.6	61	08.4	65.2	70.6	55.3	76.7	.111	NW var.	Fine—light clouds—brisk wind throughout the day. Ev. Fine & clear.
T 17	30.252	30.248	79.6	30.144	30.136	69.6	59	09.2	65.9	67.8	54.3	79.0		S	Fine—light clouds and wind throughout the day. Evening, Overcast—light wind.
W 18	30.116	30.110	67.0	30.226	30.218	69.0	60	06.8	63.8	67.2	60.0	73.5		NW	A.M. Lightly overcast—lt. wind. P.M. Fine—lt. clouds. Ev. Cloudy.
T 19	30.266	30.260	70.7	30.150	30.144	70.2	60	09.6	66.8	74.4	57.2	72.8		S var.	Fine—light clouds and wind throughout the day. Ev. Cloudy.
F 20	30.012	30.004	65.5	30.002	29.996	69.4	59	05.6	62.0	69.8	55.8	74.3	.041	W	A.M. Overcast—light wind—rain early. P.M. Fine—light clouds. Evening, Cloudy.
● S 21	30.050	30.046	66.2	30.138	30.132	66.4	55	07.0	59.4	64.0	53.2	71.3	.008	NW	A.M. Overcast—light brisk wind—rain during the night. P.M. Fine—light clouds and wind. Evening, Cloudy.
⊙ 22	30.200	30.194	74.7	30.184	30.176	65.4	52	08.5	56.0	60.0	52.3	68.4		NW var.	Cloudy—light brisk wind throughout the day. Evening, Fine & clear.
M 23	30.184	30.176	63.4	30.122	30.114	62.8	53	06.6	58.4	59.7	50.4	61.8		NW	A.M. Cloudy—light wind. P.M. Light shower. Evening, Overcast—very light rain and wind.
T 24	29.988	29.982	66.2	29.994	29.988	64.0	50	06.5	56.4	61.4	53.8	63.3	.022	NW var.	Dark heavy clouds, with brisk wind throughout the day. Evening, Cloudy—light wind.
W 25	30.008	30.002	67.7	30.024	30.016	61.8	53	08.3	59.8	54.2	49.6	62.8		W	A.M. Cloudy—brisk wind. P.M. Overcast—light rain. Ev. Overcast.
T 26	29.982	29.976	69.9	29.878	29.870	64.3	51	08.7	60.7	63.7	49.7	65.5		NW	A.M. Fine—light clouds and wind. P.M. Cloudy—light wind. Ev. Overcast—light rain.
F 27	29.820	29.816	77.4	29.834	29.826	64.9	54	09.5	60.0	65.3	52.7	66.7	.102	W	A.M. Fine—light clouds and high wind. P.M. Cloudy—high wind. Evening, Overcast—very light rain.
S 28	29.822	29.818	72.8	29.758	29.750	65.8	54	08.5	61.8	65.6	53.3	70.4	.013	NW	A.M. Fine—light clouds and wind. P.M. Overcast—light shower. Evening, Overcast—light rain.
⊙ 29	29.648	29.644	65.0	29.588	29.584	65.6	56	06.6	60.7	63.6	53.3	68.3	.052	S	A.M. Cldy.—lt. wind. P.M. Overcast—lt. rain. Ev. Rainbow—lt. rain.
M 30	29.708	29.702	75.8	29.700	29.694	64.2	55	08.4	61.4	61.4	50.2	71.3	.125	SW var.	A.M. Cloudy—light rain & wind. P.M. Showery. Ev. Overcast.
T 31	29.884	29.880	74.3	29.898	29.892	64.9	55	08.1	61.7	67.7	50.0	70.5	.208	S	Fine—light clouds—brisk wind throughout the day. Evening, Fine and clear.
MEAN.	30.022	30.015	70.1	30.007	30.001	67.5	57.8	07.3	63.3	68.1	56.0	70.8	Sum. 2.259		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 29.915 .. 29.906; C. 29.907 .. 29.899
AUGUST															
W 1	30.058	30.052	70.7	30.022	30.018	67.2	56	08.0	63.7	69.4	53.5	71.5		S	Cloudy—lt. breeze throughout the day. Ev. Overcast—lt. steady rain.
T 2	29.844	29.838	64.6	29.766	29.758	65.8	60	06.4	66.6	65.7	59.2	71.0	.133	SW var.	A.M. Dark heavy clouds, with brisk wind. P.M. Overcast—lt. rain. Evening, Continued light rain.
F 3	29.738	29.734	67.4	29.756	29.750	67.6	60	06.6	64.7	70.6	58.7	69.0	.061	SW var.	A.M. Cloudy—brisk wind. P.M. Fine—lt. clouds & wind. Ev. Overcast—light rain.
S 4	29.690	29.682	67.6	29.616	29.612	68.2	62	06.8	66.8	68.7	61.7	71.3	.013	S var.	A.M. Lightly overcast—brisk wind. P.M. Cloudy—brisk wind. Ev. Overcast—light rain—hail and wind.
○⊙ 5	29.574	29.566	65.8	29.540	29.536	67.8	59	06.5	63.3	68.7	58.0	70.2	.063	S	A.M. Overcast—light brisk wind. P.M. Fine—light clouds—brisk wind. Evening, Overcast—light wind.
M 6	29.524	29.520	69.7	29.524	29.520	67.3	60	06.2	63.8	65.5	58.8	68.7	.088	S	A.M. Dark heavy clouds—high wind. P.M. Fine—lt. clouds & wind. Evening, Fine and clear.
T 7	29.732	29.730	61.7	29.784	29.780	67.8	57	07.0	62.5	65.3	55.0	63.0	.338	W	A.M. Fine—lt. clouds—brisk wind. P.M. Overcast—brisk wind. Evening, Light rain.
W 8	30.056	30.050	66.0	30.116	30.110	66.0	60	07.0	60.5	66.8	54.0	68.0	.066	W	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear—brisk wind.
T 9	30.236	30.232	66.2	30.170	30.166	66.0	58	07.2	63.5	67.3	51.0	67.0		SSW	A.M. Fine—light clouds. P.M. Overcast—brisk wind. Evening, Fine and clear—brisk wind.
F 10	30.074	30.070	64.0	30.070	30.066	68.0	60	04.5	63.8	70.2	58.0	69.0		SW var.	Overcast—brisk wind throughout the day.
S 11	30.096	30.092	67.0	30.100	30.096	68.0	62	06.2	68.4	72.2	62.0	72.0		W var.	A.M. Overcast—brisk wind. P.M. Fine—light clouds—brisk wind. Evening, Overcast—brisk wind.
⊙ 12	30.120	30.116	69.0	30.080	30.076	69.0	63	05.8	67.0	75.5	61.0	72.5		SW	A.M. Overcast—light wind. P.M. Fine—light clouds—brisk wind. Evening, The like.
M 13	30.094	30.090	67.9	30.110	30.104	71.0	63	05.6	65.0	70.9	62.0	75.6		W	A.M. Overcast—light wind. P.M. Fine—light clouds—brisk wind. Evening, The like.
T 14	30.262	30.260	70.2	30.190	30.186	67.2	56	05.8	60.6	67.5	51.0	71.0		NW	Fine—light clouds and wind throughout the day.
W 15	30.158	30.152	69.7	30.154	30.150	67.4	58	07.4	61.2	65.4	53.0	68.5		N	A.M. Fine—light clouds—brisk wind. P.M. Overcast—brisk wind. Evening, Fine—light clouds—brisk wind.
T 16	30.146	30.142	66.1	30.076	30.070	66.0	58	05.1	62.0	69.4	53.5	67.0		WSW	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Overcast.
F 17	30.190	30.184	63.0	30.206	30.202	63.2	55	05.6	58.0	61.7	55.0	70.0		NNE	Overcast—light wind throughout the day. Evening, Rain.
S 18	30.254	30.250	64.3	30.206	30.200	65.5	60	03.9	62.5	69.9	58.0	62.5	.044	WSW	Overcast throughout the day.
⊙ 19	30.012	30.008	66.2	29.904	29.900	67.0	62	06.5	67.4	67.8	59.5	70.6		W	A.M. Fine—light clouds and wind. P.M. Overcast—light rain—brisk wind. Evening, Light clouds—brisk wind.
● M 20	29.832	29.826	65.0	29.708	29.704	66.0	57	04.8	61.2	67.2	60.6	73.0	.008	SW	A.M. Overcast—lt. wind. P.M. Fine—lt. clds. Ev. Overcast—lt. wind.
T 21	29.358	29.354	69.2	29.384	29.380	67.0	60	05.6	65.5	65.5	59.5	69.0		W var.	Fine—light clouds—high wind, with occasional showers during the day. Ev. Heavy clds.—h. wind. High wind throughout the night.
W 22	29.252	29.248	64.2	29.242	29.238	65.0	55	06.5	60.2	62.0	54.2	69.0	.050	WSW	Overcast—high wind, with occasional showers throughout the day. High wind, with rain during the night.
T 23	29.468	29.464	63.0	29.568	29.564	64.0	56	05.4	59.0	64.5	54.5	65.0	.088	NNW	A.M. Fine—lt. clds.—brisk wind—11½, thunder & lightning, with rain & h. wind. Fine—lt. clds. rest of the day. Ev. Cldy.—lt. wind—rain during night.
F 24	29.966	29.960	63.0	30.012	30.008	65.0	57	06.0	59.0	61.5	51.2	65.0	.019	NNW	Overcast—lt. brisk wind throughout the day.
S 25	30.102	30.096	64.0	30.052	30.048	62.2	54	03.9	57.0	60.0	49.7	63.5		WSW	Overcast—light wind, with occasional rain throughout the day.
⊙ 26	30.108	30.102	64.0	30.092	30.088	64.3	58	04.0	60.2	68.6	54.0	65.5	.066	NW	A.M. Overcast. P.M. Fine—light clouds—brisk wind. Ev. The like.
M 27	30.132	30.128	65.0	30.114	30.108	66.0	60	04.0	68.0	75.0	60.0	69.0	.013	NW	Fine—light clouds and wind throughout the day.
T 28	30.086	30.080	66.5	29.948	29.944	69.0	65	03.0	67.0	73.0	64.0	76.0		SSW	A.M. Lightly overcast—light fog. P.M. Fine—light clouds. Evening, Fine—light clouds and wind.
W 29	29.830	29.826	68.5	30.014	30.010	66.0	60	06.0	62.4	59.5	61.0	76.0		N var.	Fine and clear—brisk wind throughout the day.
T 30	30.190	30.186	63.5	30.134	30.130	63.0	51	05.0	55.4	65.3	46.5	62.5		N	Fine—light clouds and wind throughout the day.
F 31	30.190	30.186	63.4	30.126	30.120	64.0	55	05.0	60.6	68.4	50.3	65.8		WNW	Fine—light clouds and wind throughout the day.
MEAN.	29.947	29.943	66.0	29.929	29.924	66.4	58.6	05.7	62.8	67.4	56.4	69.0	Sum. 1.050		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 29.851 .. 29.831; C. 29.846 .. 29.825

The observations for the month of August were not taken by the Assistant Secretary, on account of absence.

METEOROLOGICAL JOURNAL FOR SEPTEMBER AND OCTOBER, 1838.

1838.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	Crown Glass.	Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	Crown Glass.	Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Ther.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest	Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
SEPTEMBER S 1	30.108	30.102	62.2	30.070	30.066	64.0	56	04.5	61.0	67.5	54.0	69.5		W	A.M. Lightly overcast. P.M. Fine—light clouds. Ev. The same.
⊙ 2	30.140	30.136	63.0	30.142	30.136	64.4	55	06.0	61.5	65.2	57.0	68.5	.030	NW	A.M. Overcast. P.M. Fine—light clouds. Evening, The same.
M 3	30.190	30.186	62.6	30.128	30.122	64.0	53	05.0	60.0	66.5	53.0	68.0		SW	A.M. Lightly overcast. P.M. Fine—light clouds. Evening, The same, with light wind.
○ T 4	29.998	29.994	65.5	29.854	29.850	65.0	55	04.2	59.0	67.2	52.0	69.0		SSW	A.M. Ovrct.—lt. fog. P.M. Fine—lt. clds.—brisk wind. Ev. The same.
W 5	29.612	29.608	65.2	29.516	29.512	65.2	61	04.8	64.0	68.7	59.8	69.0		S	A.M. Overcast—brisk wind. P.M. Fine—light clouds—high wind. Evening, The same.
T 6	29.290	29.286	63.6	29.272	29.268	65.0	59	02.2	59.2	64.2	58.0	69.0	.372	SSW	A.M. Overcast—rain early. P.M. Dark heavy clouds—brisk wind. Evening, Overcast—brisk wind.
F 7	29.344	29.340	64.5	29.332	29.328	65.0	60	05.5	63.5	66.0	57.4	64.5	.050	S	A.M. Heavy clouds—light brisk wind. P.M. Lightly overcast—brisk wind. Evening, Heavy shower—brisk wind.
S 8	29.640	29.636	60.0	29.780	29.776	61.0	51	03.0	52.0	55.2	52.0	68.0	.036	NNE	Overcast—brisk wind throughout the day.
⊙ 9	30.220	30.214	57.0	30.262	30.258	58.5	46	03.5	51.5	58.3	44.5	56.0		SW	Overcast—light fog and wind throughout the day.
M 10	30.406	30.400	57.0	30.414	30.408	58.0	49	04.0	51.5	58.4	46.5	59.5		NW	Overcast—light fog and wind throughout the day.
T 11	30.548	30.544	55.5	30.510	30.504	56.2	47	02.0	48.7	59.3	45.0	59.5		NW	Lightly overcast, with light fog throughout the day.
W 12	30.500	30.496	56.5	30.404	30.400	57.0	50	03.0	53.2	64.5	47.5	60.0		SW	A.M. Overcast. P.M. Fine—light clouds the remainder of the day.
T 13	30.328	30.324	58.0	30.252	30.248	59.0	53	03.7	57.8	63.2	51.5	65.0		W	Overcast throughout the day.
F 14	30.146	30.142	60.2	30.100	30.096	60.0	55	05.0	61.0	64.4	52.5	64.0		SW	Lightly overcast, with light wind nearly the whole of the day.
S 15	30.088	30.084	60.0	30.050	30.042	61.8	59	02.0	59.5	65.2	59.0	65.0		NW	Overcast—light fog and wind. Evening, Fine and clear.
⊙ 16	30.078	30.072	61.4	30.028	30.020	61.2	55	06.0	55.8	66.7	50.8	65.2		N	A.M. Light fog and wind. P.M. Fine—light clouds. Ev. Cloudy.
M 17	30.072	30.066	60.6	30.042	30.034	62.7	55	04.5	59.3	64.9	55.3	67.7		NNE	Fine—light clouds and wind nearly the whole of the day. Ev. Cloudy.
● T 18	30.056	30.050	59.9	30.006	30.000	61.3	54	05.7	57.3	59.5	52.4	67.8		NE	Cloudy—light brisk wind nearly the whole of the day. Ev. Overcast.
W 19	29.948	29.940	58.7	29.886	29.880	59.6	53	03.1	54.4	57.4	54.6	60.4		NNW	Overcast—very light rain and wind throughout the day.
T 20	29.858	29.850	60.2	29.852	29.844	60.9	54	04.6	57.6	61.4	54.6	58.6	.022	SW	A.M. Overcast—light rain and wind. P.M. Cloudy. Ev. Fine & clear.
F 21	29.850	29.846	58.2	29.848	29.840	59.8	49	03.0	50.7	59.3	46.3	63.8		S	A.M. Light fog and wind. P.M. Fine—light clouds and wind. Evening, Fine starlight night.
S 22	30.000	29.992	58.3	29.968	29.960	57.8	50	04.7	53.7	60.4	42.8	60.2		SSW	A.M. Fine—light fog and wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
⊙ 23	29.918	29.910	57.3	29.892	29.886	60.2	54	05.3	57.6	63.5	52.7	60.6		S	A.M. Overcast—very lt. rain. P.M. Ovrct.—brisk wind. Ev. Fine rain.
M 24	29.918	29.910	57.9	29.826	29.818	57.9	53	04.2	54.7	53.3	51.0	63.7	.038	N	A.M. Overcast—very light rain and wind. P.M. Light steady rain—high wind. Evening, The same.
T 25	29.820	29.814	56.9	29.836	29.830	57.9	52	02.7	51.6	55.2	50.4	56.3	.750	W	Overcast, with occasional light mist nearly the whole of the day. Evening, Cloudy.
W 26	29.988	29.980	55.2	29.980	29.972	57.0	52	01.7	48.8	57.7	47.3	55.3		SSW	A.M. Thick fog. P.M. Overcast. Evening, Light fog.
T 27	29.864	29.856	56.5	29.852	29.846	56.7	53	00.9	52.6	53.8	49.2	58.3	.116	NW	A.M. Overcast—light rain and wind. P.M. Overcast—brisk wind. Evening, Fine starlight night.
F 28	30.000	29.996	54.4	30.004	29.996	55.7	47	01.9	46.3	60.2	45.0	54.7	.783	S	A.M. Thick fog—deposition. P.M. Fine—nearly cloudless. Evening, Cloudy.
S 29	30.016	30.010	55.9	30.016	30.010	56.7	53	02.9	56.3	58.2	46.5	61.0		N	Overcast—very fine rain nearly the whole of the day.
⊙ 30	30.200	30.192	57.6	30.218	30.212	59.0	53	03.1	57.8	60.4	56.4	59.2	.050	NW	Lightly overcast nearly the whole of the day. Evening, Light fog.
MEAN.	30.005	29.999	59.3	29.978	29.965	60.3	53.2	03.8	55.9	61.5	51.5	62.6	Sum. 2.247		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 29.923 .. 29.897 C. 29.920 .. 29.883
OCTOBER M 1	30.338	30.332	56.3	30.308	30.300	57.2	52	02.4	51.8	56.5	52.6	60.6		NW	Overcast—brisk wind throughout the day. Evening, Cloudy.
T 2	30.356	30.350	56.4	30.360	30.352	58.4	52	03.4	55.4	59.7	52.2	57.6		N	A.M. Overcast—brisk wind. P.M. Cloudy—brisk wind. Evening, Fine—light clouds.
○ W 3	30.428	30.420	55.5	30.394	30.386	56.5	50	04.5	55.7	60.6	47.8	60.4		NE	Fine and cloudless—brisk wind throughout the day. Ev. Fine & clear.
T 4	30.394	30.388	54.3	30.362	30.354	56.0	48	03.1	51.0	58.5	53.2	61.2		N	Fine—light clouds & wind throughout the day. Ev. Fine & clear.
F 5	30.352	30.346	52.7	30.334	30.326	54.3	48	02.7	49.7	56.3	46.6	58.8		N	A.M. Overcast—brisk wind. P.M. Fine and cloudless. Ev. Cloudy.
S 6	30.372	30.366	52.3	30.354	30.346	53.4	45	04.0	49.8	53.7	46.9	56.6		NNE	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
⊙ 7	30.372	30.364	53.8	30.318	30.312	55.6	47	03.1	51.4	53.4	49.2	54.5		NNW	Overcast—brisk wind, with occasional fine rain nearly the whole of the day.
M 8	30.352	30.346	52.9	30.332	30.324	54.6	47	05.0	52.0	54.7	48.8	54.9		NW	Cloudy—light wind throughout the day. Evening, Very fine rain.
T 9	30.360	30.352	53.3	30.296	30.288	55.0	48	03.5	52.4	57.2	49.0	55.7		NW	Cloudy—light brisk wind throughout the day.
W 10	30.230	30.224	53.4	30.158	30.152	54.5	47	03.9	51.3	53.7	50.0	57.6		WNW	Overcast—light fog and wind nearly the whole day. Ev. Cloudy.
T 11	29.996	29.990	53.7	29.788	29.782	55.8	48	03.4	52.8	58.5	49.9	54.3		SSW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. (Sudden fall of the Barom.) Evening, Cloudy—high wind.
F 12	29.652	29.648	53.5	29.650	29.644	53.8	44	04.9	46.2	47.4	42.8	59.7		W	Fine—nearly cloudless—brisk wind the whole day. High wind during night. Ev. Fine and clear.
S 13	29.762	29.754	47.4	29.810	29.802	49.4	39	03.1	39.4	39.8	36.4	50.3		W	A.M. Fine & cloudless—lt. wind. Sharp frost during night. P.M. Ovct.—lt. snow & rain. Ev. Cloudy.
⊙ 14	29.904	29.896	45.3	29.766	29.758	46.9	38	03.9	41.4	47.3	32.8	45.0		S	Overcast—very light rain and wind throughout the day. Evening, Continued lt. rain, with high wind.
M 15	29.574	29.568	48.7	29.626	29.618	50.3	43	04.6	51.8	54.9	40.4	53.8	.075	W	A.M. Fine—lt. clouds and wind. P.M. Cloudy—lt. wind. Ev. Cloudy—high wind.
T 16	29.614	29.612	52.6	29.546	29.542	54.6	48	03.8	55.7	60.4	48.8	59.7		SW var.	A.M. Overcast—high wind. P.M. Cloudy—high wind. Evening, Overcast—light rain—high wind.
W 17	29.292	29.288	55.3	29.358	29.352	57.2	50	03.5	54.5	57.3	53.5	61.0	.125	S	Fine—light clouds and wind throughout the day. High wind throughout the night. Evening, Fine and starlight.
● T 18	30.054	30.048	51.7	30.014	30.008	53.5	41	03.8	46.2	52.7	42.0	59.3		SW var.	A.M. Fine & cloudless—lt. wind. P.M. Overcast—lt. rain & wind. Ev. The like, with high wind.
F 19	29.910	29.902	54.5	30.028	30.022	55.7	50	03.6	57.8	57.3	46.2	58.4	.033	SW var.	A.M. Dark heavy clouds—high wind. P.M. Fine—nearly cloudless—lt. wind. Ev. Cloudy.
S 20	30.088	30.080	55.0	30.138	30.130	57.5	51	03.4	57.8	62.3	54.0	59.3		SW	A.M. Overcast—very fine rain—light wind. P.M. Fine—lt. clouds and wind. Evening, Fine and starlight.
⊙ 21	30.296	30.292	55.3	30.254	30.250	57.0	52	03.6	55.8	60.5	48.4	63.5		S	Overcast—light mist nearly the whole of the day.
M 22	30.156	30.150	56.8	30.094	30.088	58.3	52	04.0	57.4	59.9	55.7	61.2		SE	A.M. Overcast—light wind. P.M. Cloudy the remainder of the day.
T 23	29.960	29.952	57.0	29.874	29.868	58.3	53	04.1	56.7	56.8	54.8	61.0		S	Overcast—light wind throughout the day. Rain at night.
W 24	29.740	29.736	57.4	29.842	29.836	58.5	52	03.3	55.0	58.2	53.7	58.6	.083	S	Fine—lt. clds. nearly the whole day. Ev. Cloudy, with occasional rain.
T 25	30.118	30.110	55.6	30.054	30.048	57.3	50	02.5	50.7	57.3	46.0	59.9	.013	SW	Dark heavy clds. nearly the whole day. Ev. The like with high wind.
F 26	29.780	29.774	56.6	29.848	29.842	57.9	52	03.7	57.3	57.9	49.0	58.2		SW	A.M. Dark heavy clouds—brisk wind. High wind throughout the night. P.M. Fine—light clouds & wind. Ev. Fine & clear.
S 27	29.922	29.914	53.2	29.762	29.756	55.3	47	03.9	49.7	55.4	41.3	59.3		S	A.M. Fine—lt. clds. & wind. P.M. Dark heavy clds.—h. wind. Ev. Cloudy—lt. rain—high wind.
⊙ 28	29.598	29.592	54.7	29.536	29.532	55.0	48	02.2	51.6	52.5	41.2	56.6	.488	W	A.M. Fine—lt. clouds & wind. P.M. Ovct. Ev. Steady rain. Heavy rain—high wind during night.
M 29	29.356	29.350	53.6	29.550	29.544	54.4	47	04.7	49.2	51.4	43.7	57.2	.330	SW var.	A.M. 3 to 5½, a hurricane, with fine starlight morn, after which the wind somewhat abated—rest of the day fine & clear. Ev. The same.
T 30	29.746	29.740	50.9	29.722	29.716	52.6	44	03.1	46.3	51.4	40.0	52.7		E	Fine and cloudless—light wind throughout the day. Ev. Cloudy.
W 31	29.824	29.816	49.6	29.812	29.804	49.5	43	00.5	43.3	43.3	40.0	52.0	.213	N	A.M. Overcast—light steady rain & wind. P.M. Cloudy. Ev. Cloudy.
MEAN.	29.993	29.990	53.5	29.977	29.970	55.0	47.6	03.5	51.5	55.1	47.0	57.4	Sum. 1.360		Mean Barometer corrected.............. 9 A.M. 3 P.M. F. 29.930 .. 29.910 C. 29.926 .. 29.902

METEOROLOGICAL JOURNAL FOR NOVEMBER AND DECEMBER, 1838.

1838.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point, at 9 A.M. deg. Fahr.	Diff. of Wet and Dry Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering. Lowest.	External Thermometers. Self-registering. Highest.	Rain in Inches. Meas. at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
T 1	29.420	29.416	48.3	29.300	29.294	49.3	42	03.5	48.7	47.7	39.6	49.2	.383	W	A.M. Cloudy—light wind—heavy rain early. P.M. Fine—light clouds. Evening, Fine and clear.
○ F 2	29.358	29.352	46.2	29.250	29.246	47.6	40	02.7	41.7	46.8	38.7	51.0	.077	S	A.M. Fine—light clouds with occasional rain. P.M. Fine—light clouds and wind. Evening, Cloudy—light rain.
S 3	29.464	29.458	44.8	29.300	29.246	45.9	38	02.6	39.8	45.5	36.4	48.2	.038	SSW	Overcast—lt. brisk wind throughout the day. Ev. Light steady rain.
⊙ 4	28.848	28.844	45.9	28.800	28.796	48.0	41	03.2	45.6	49.5	40.6	47.9	.133	S	A.M. Cloudy—light fog and wind. Light rain, with rainbow. Evening, Overcast—very light rain.
M 5	29.156	29.150	46.8	29.306	29.300	47.9	42	02.4	46.4	49.4	40.7	51.2	.050	SW	Overcast—light rain & wind throughout the day. Ev. The same.
T 6	29.722	29.716	45.8	29.710	29.704	46.8	38	01.8	40.4	48.3	38.9	50.3		W	A.M. Fine and cloudless—light fog and wind. P.M. Fine—light clouds. Evening, Rain, with very high wind.
W 7	29.462	29.456	48.8	29.424	29.416	51.9	45	02.2	53.7	56.7	40.9	54.4	.166	SE var.	A.M. Overcast—lt. rain—high wind. Very high wind during the night. P.M. Fine—lt. clouds & wind. Ev. Light rain—high wind.
T 8	29.554	29.550	51.6	29.534	29.528	52.2	46	03.3	50.4	53.8	49.3	57.7	.011	SW var.	A.M. Fine & cloudless—lt. wind. P.M. Cloudy. Ev. Overcast—lt. rain.
F 9	29.370	29.364	50.8	29.438	29.432	51.4	45	02.4	46.3	49.8	46.3	54.7	.250	WNW	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, Fine and starlight.
S 10	29.708	29.702	47.4	29.746	29.740	48.3	42	02.9	43.8	46.8	39.5	50.3	.061	SW	A.M. Fine—light clouds and wind. P.M. Cloudy. Ev. Fine & starlight.
○ 11	29.654	29.648	44.3	29.604	29.600	44.2	36	01.2	33.4	40.5	33.6	48.0		W	A.M. Thick fog—lt. wind. P.M. Overcast—very lt. rain. Ev. Ditto.
NOVEMBER M 12	30.096	30.090	41.7	30.176	30.168	43.4	35	02.8	37.9	45.0	37.3	41.6		N	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
T 13	30.424	30.416	40.8	30.384	30.378	42.4	36	02.6	37.9	46.8	35.7	46.6		N	Fine and cloudless—light wind throughout the day. Evening, Fine and starlight.
W 14	30.250	30.242	39.9	30.134	30.126	41.8	34	01.7	37.8	45.2	35.3	47.7		N	Fine and cloudless—light fog with brisk wind nearly the whole of the day. Evening, Overcast—deposition.
T 15	29.924	29.918	42.3	29.816	29.808	43.0	38	01.5	42.0	44.4	38.2	46.7		E	Thick fog—deposition throughout the day. Evening, Overcast.
F 16	29.698	29.694	44.8	29.658	29.652	45.6	41	02.1	43.7	46.3	41.8	45.2		E	Overcast—light fog and wind nearly the whole day. Ev. Thick fog.
● S 17	29.746	29.740	44.7	29.720	29.714	45.4	38	01.5	41.8	45.8	41.0	46.7		S	Light fog throughout the day. Ev. Overcast—light steady rain.
⊙ 18	29.632	29.624	45.2	29.650	29.646	46.0	40	01.0	43.7	44.6	42.0	46.7	.261	N	Overcast—lt. rain and wind throughout the day. Ev. Continued rain.
M 19	29.566	29.560	44.8	29.480	29.474	43.7	39	00.5	40.0	38.7	40.8	45.0	.327	NE	Overcast—light rain—high wind throughout the day. Ev. The like.
T 20	29.656	29.650	41.9	29.668	29.660	42.3	36	02.1	38.8	39.8	37.8	39.8	.116	NNE	Overcast—high wind throughout the day. Evening, The same.
W 21	29.410	29.404	40.9	29.324	29.320	41.3	36	01.0	38.7	40.3	36.4	40.7		NE	Overcast—brisk wind throughout the day. Evening, Light rain.
T 22	29.166	29.160	42.7	29.212	29.206	44.7	40	00.8	44.4	47.3	38.4	45.3	.122	NE	A.M. Overcast—light fog—rain & wind. P.M. Cloudy. Evening, The same.
F 23	29.514	29.506	44.9	29.528	29.524	45.0	40	02.0	43.7	45.7	42.7	47.5	.033	NE	A.M. Cloudy—lt. brisk wind. P.M. Overcast. Ev. Light fog.
S 24	29.568	29.562	41.8	29.636	29.632	41.7	35	03.1	38.8	38.7	37.6	44.3		NNE	Overcast—brisk wind throughout the day. Evening, Fine & clear—sharp frost.
⊙ 25	29.994	29.986	37.9	30.004	30.000	38.0	34	02.1	33.3	38.0	32.4	39.6		N	A.M. Light fog. P.M. Overcast. Ev. Fine and clear—sharp frost.
M 26	29.880	29.874	36.2	29.888	29.882	38.7	28	02.0	32.7	32.4	32.5	37.8		E	A.M. Cloudy—brisk wind—sharp frost. P.M. Fine—nearly cloudless—light wind. Ev. Fine and clear—sharp frost.
T 27	29.490	29.484	34.6	29.254	29.250	35.4	30	*id. fro.*	33.8	36.3	30.0	34.3		ENE	Lightly overcast—brisk wind throughout the day. Ev. Light rain.
W 28	29.040	29.036	38.2	28.650	28.644	40.2	35	01.5	41.8	46.7	33.8	42.5	.041	E	A.M. Cloudy—light rain and wind. P.M. Overcast—high wind. Evening, Light rain with very high wind.
T 29	29.658	29.654	46.3	28.678	28.674	47.3	42	02.2	48.7	50.3	41.8	51.7	.216	S	Cloudy—very high wind, with rain nearly the whole day. At 4 to 10 P.M. thunder & lightening, with rain & very high wind.
F 30	28.958	28.952	46.8	29.158	29.150	48.2	42	02.3	47.6	51.3	46.4	52.0	.450	SE	A.M. Fine and serene, with light wind. Ev. Fine & clear. At 4 P.M. 10 o'clock, very heavy hail storm, accompanied with high wind.
MEAN.	29.580	29.577	43.9	29.514	29.507	44.9	38.5	02.1	41.9	45.3	38.9	46.8	Sum. 2.735		Mean Barometer corrected ..... 9 A.M. F. 29.543 .. 29.475; 3 P.M. C. 29.539 .. 29.467
○ S 1	29.552	29.544	47.2	29.520	29.514	48.4	42	01.3	46.7	51.2	45.2	52.0		S	A.M. Fine—light clouds and wind. P.M. Overcast—light wind. Rain, with very high wind.
⊙ 2	29.464	29.458	50.8	29.484	29.476	51.6	46	02.9	52.7	52.6	46.2	53.2	.144	S	A.M. Fine—lt. clds. & wind. P.M. Over.—lt. rain & wind. Ev. Over. & before 11, *Thunder & lightening, with heavy rain & high wind.*
M 3	29.464	29.456	49.3	29.442	29.436	49.9	44	01.9	46.7	49.7	46.0	55.0	.577	S	Fine & cloudless—lt. wind throughout the day. Ev. Fine & clear.
T 4	29.564	29.556	47.6	29.610	29.602	48.4	41	01.3	42.2	47.6	42.5	48.8		SW	A.M. Fine & cloudless—light fog. P.M. Cloudy. Evening, Fine starlight night.
W 5	29.834	29.828	45.2	29.928	29.922	46.3	40	01.5	40.9	45.8	38.9	48.6		W	A.M. Overcast—light fog—deposition. P.M. Fine—light clouds and wind. Evening, Overcast.
T 6	30.256	30.250	45.3	30.286	30.278	45.9	36	02.4	41.8	42.7	40.8	47.2	.022	S	A.M. Light fog. P.M. Fine—light clouds & wind. Ev. Overcast.
F 7	30.256	30.248	45.9	30.274	30.266	46.7	43	01.5	47.7	47.3	40.5	48.6	.158	NW	Overcast—light rain throughout the day. Evening, Fine and clear.
S 8	30.362	30.354	43.3	30.368	30.360	43.7	37	02.7	38.8	42.2	37.6	48.7	.102	NW	Fine—very light clouds and wind throughout the day. Evening, Fine and clear.
⊙ 9	30.412	30.404	40.3	30.370	30.364	41.5	33	01.5	33.8	37.4	32.9	41.0		NW	A.M. Thick fog—deposition—light wind. P.M. Overcast. Evening, Thick fog—sharp frost.
M 10	30.280	30.272	38.4	30.208	30.200	38.8	32	02.0	33.7	38.7	30.0	38.3		NW	Overcast—light fog nearly the whole of the day. Evening, Fine and starlight.
DECEMBER T 11	30.288	30.282	39.2	30.316	30.308	39.9	34	01.8	39.9	43.7	33.7	40.6		SW	Thick fog, with light wind throughout the day. Ev. Overcast.
W 12	30.342	30.334	40.9	30.300	30.292	41.7	36	02.2	41.3	44.2	39.6	44.3		SW	Overcast—light fog throughout the day, as also the evening.
T 13	30.326	30.320	42.2	30.330	30.322	42.9	38	02.2	43.2	45.8	41.6	44.7		S	Overcast—light fog throughout the day, as also the evening.
F 14	30.410	30.402	44.7	30.384	30.376	45.2	38	02.2	42.8	45.8	42.8	45.5		NW	A.M. Thick fog. P.M. Fine—light clouds and wind. Ev. Light fog.
S 15	30.366	30.358	44.2	30.318	30.310	44.6	38	02.0	40.6	41.8	41.0	46.2		ENE	A.M. Overcast—light rain and wind. P.M. Fine—light clouds. Evening, Fine and starlight.
⊙ 16	30.360	30.352	41.8	30.358	30.350	42.0	33	01.7	38.4	39.2	38.2	43.3		E	A.M. Cloudy—light wind. P.M. Overcast. Evening, Light fog.
● M 17	30.366	30.360	40.5	30.332	30.324	40.3	34	02.4	35.6	35.8	35.4	39.8		SE	Overcast—brisk wind throughout the day, as also the evening.
T 18	30.316	30.308	39.2	30.280	30.272	39.8	32	02.5	35.3	35.3	34.9	36.6		SSE	Overcast—light wind, with sharp frost throughout the day, as also the evening.
W 19	30.224	30.218	37.8	30.172	30.164	38.2	30	02.1	34.6	37.5	33.4	36.3		SSE	Overcast—light wind throughout the day. Ev. Fine & clear.
T 20	30.192	30.184	37.6	30.206	30.200	38.9	32	01.8	36.7	41.7	34.9	37.3		S	A.M. Cloudy—thick fog. P.M. Fine—light clouds. Ev. Fine & starlight.
F 21	30.364	30.358	40.9	30.354	30.346	40.6	35	00.8	35.8	36.7	35.7	36.7		SE	Overcast—light brisk wind throughout the day, as also the evening.
S 22	30.104	30.096	39.3	30.016	30.010	39.4	33	01.9	35.8	38.0	34.2	38.3		S	A.M. Lightly overcast—light wind. P.M. Overcast—light rain. Evening, Continued rain.
⊙ 23	29.572	29.568	41.2	29.444	29.438	42.0	36	02.0	42.8	43.8	36.0	43.7	.311	E	Overcast—very light rain and wind nearly the whole of the day. Evening, Overcast—light wind.
M 24	29.310	29.304	42.9	29.274	29.268	42.9	37	01.5	41.8	39.2	41.8	44.4	.047	E	Ditto Ditto. Evening, Overcast.
T 25	29.658	29.650	40.6	29.814	29.810	41.2	32	02.1	34.5	35.8	34.2	35.7	.016	NW	Fine—light clouds throughout the day. Evening, Fine and clear. *Sharp frost during the night.*
W 26	29.918	29.914	36.2	29.734	29.728	37.2	27	01.0	29.7	38.2	29.8	34.8		S	A.M. Light fog. P.M. Over—very lt. rain. Ev. Lt. rain, with b. wind.
T 27	29.662	29.654	37.9	29.776	29.770	39.2	31	01.5	34.8	41.7	30.0	39.2	.147	SW	Fine—light clouds throughout the day. Evening, Fine and clear. *Sharp frost during the night.* (Fine & clear.
F 28	30.230	30.222	37.4	30.298	30.292	38.2	31	01.3	33.8	40.6	33.8	40.8		W	A.M. Fine—lt. fog—*white frost.* P.M. Fine—lt. clouds & wind. Ev.
S 29	30.222	30.216	37.4	30.196	30.188	39.3	33	02.2	39.9	43.7	33.8	40.8		S	A.M. Cloudy—light rain & wind. P.M. Cloudy—light wind. Evening, Overcast—very light rain.
⊙ 30	30.152	30.146	42.5	30.066	30.058	44.4	38	01.0	47.4	49.7	40.2	48.2	.033	S	Overcast—very light rain and wind nearly the whole of the day, as also the evening.
○ M 31	30.496	30.490	42.6	30.534	30.528	42.8	35	01.7	36.8	41.7	36.2	50.7	.044	WNW	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
MEAN.	30.075	30.068	41.9	30.064	30.057	42.6	35.7	01.8	39.6	42.4	37.5	43.5	Sum. 1.601		Mean Barometer corrected ..... 9 A.M. F. 30.042 .. 30.030; 3 P.M. C. 30.034 .. 30.022

Note.—At half-past Two a.m. of the 29th of November, the Barometer stood 29.600 F., 29.594 C., at which time there was a severe gale, accompanied with thunder and lightning. After Three the sky cleared, but wind still raging; and at Four, again overcast, with rain.